Philosophy of Science

VOL. I July, 1934 NO. 3

The Method of Physical Coincidences and the Scale Coordinate

BY

WM. BENDER

INTRODUCTION

THE history of Physical Science appears to exhibit, periodically, a race between the accumulation of data and the ability of its codification (usually, a mathematical theory) to find a natural place (in the codifying scheme) for much of the empirical findings. If the codifying scheme is a mathematical theory, capable of interpolation and extrapolation, according to the rules of the particular branch of mathematics employed, the ablest handlers of the theory are frequently confronted with a situation in which mathematical computation alone does not suffice. In such a situation the introduction of symbols, whose interpretation is not direct and simple (from the point of view of the methodology whereby the data were procured) is resorted to. This symbol is usually involved in a mathematical relation by a species of reasoning, which may be somewhat unfamiliar and remote from customary habits of thought. It is generally accepted as physically valid, if the mathematical formulation which involves it can produce

results which account for the mass of data uncovered by experimenters. As an example of such a procedure we may mention the case of the Schrödinger Ψ-symbol, a symbol which was involved in Schrödinger's famous matter wave equation. The successes which such a theory enjoyed, particularly, in its initial stages, in formally relating the relative positions and intensities of spectral lines, with the atomistic concepts of gross matter which were *inferred* from the data of macroscopic measurement, such as the atomic mass, the electronic charge, Planck quantum of action, etc.,—these successes, gave a certain physical dignity to the Ψ-function itself. This dignity was further enhanced by the disclosure, by Schrödinger, of four functions of the Ψ-symbol (and its complex conjugate) which held the same place in the "continuity equation" as the classical charge density and the three components of current density. The successes accompanying this discovery for spectroscopic problems, in particular, are well known to Physicists.

But the experimenters were not to be denied. A great number of earnest workers went on accumulating a vast mass of spectroscopic and other quantum data, in a remarkably short time, with the result that the ablest exponents of the Schrödinger Theory or of other equivalent quantum mechanical theories, currently in use, found it extremely difficult to manipulate many of the mathematical formulations involved, and in some cases, (in the present development of mathematical science) well nigh impossible. There is, too, another matter which requires comment. The new quantum mechanics has brought about a situation under which (in many cases) simple mechanico-geometrical pictures are logically relinquished. Now the intellectual difficulties in giving up classical mechanico-geometrical pictures are small, compared with the formal difficulties of computation involved, though, as a matter of historical observation, a relatively simple geometrical picture is more often than not, a decided aid in manipulating a purely formal theory.

Now, at this critical stage of an exact science, two points of view seem to arise. The first demands more time in which the mathematical difficulties may be further studied in the spirit

of a pure mathematician. Closely allied with this point of view is the attitude that by the introduction of *additional* symbols (and their functional involvements) also somewhat remote from simple interpretation in empirical method, a way out may be realized. Such attitudes have, in fact, had a degree of success. One need only mention the crescendo of aether concepts, with their attendant (increasingly involved) mathematical formulations of the aether theories between 1885 and 1905, some of which were notably successful.

At this point, it may be mentioned parenthetically, that the Special Relativity theory of 1905, which is a theory of measurement, rendered most of the concepts of the "aether particle" and their mathematical formulations, impertinent. (Contrast, too, the more highly conceptual theories of Kepler with those of Newton.) It appears to the writer that the present state of the quantum electrodynamical theories of Physics is in a situation which has a close parallel to optical theories just before 1905.

Now, a second point of view, looks for a remedy in the Methodology of Measurement. The implications of such an attitude are these: That by a closer examination of those objective criteria (which are partly configurational, partly operational) which define the concept; and also, the re-examination of the measuring process itself, new and simple *analytic elements* may be found which have a direct and unmistakable correspondence with the process on which the number-dimension data are to be had. This second attitude, frequently takes the stand that a highly involved current theory, even though it (if pushed hard enough) would render an accounting of the data, takes a disproportionate[1] amount of time and effort.

In the spirit of the second attitude aforementioned, we begin

[1] These two points of view raise the questions: What is a "good" codification; what is a "bad" codification? When it is fully appreciated that many different schemes of codification may be advantageously employed for the same set of accumulated data (the differences in the schemes of codification arising, chiefly, from the differences in the analytic elements which seem acceptible) the criteria for "good" theories and "bad" theories will probably be more frequently raised. This appears, to the writer, a problem of great moment, on account of its possible affect on the future course of the exact sciences. We require an Ethics of Codification, so to speak.

to re-examine the question of what is involved in making a measurement.[2]

PART I. FIELD COINCIDENCES AND MATTER COINCIDENCES

The Amorphous Coordinate System. The theory of Relativity is a theory of measurement which lays emphasis on:

(a) The amorphous coordinate system[3] with its attendant observer.

(b) The event (or coincidence) which is the prime physical invariant as between two observers, each with his own amorphous coordinate system.

(c) The agency, light energy, whereby the record of the event may be given spacetime position in the coordinate system.

(a), (b) and (c) imply concepts which have an intimate connection with the methodology of measurement. Now, we shall make the assertion, in the sense of an historical interpretation, that the most fruitful[1] codifying schemes arise through emphasis on the methodology of measurement. In view of this assertion one might justifiably raise the question: Why did not the Relativity Theory, which in emphasizing (a), (b) and (c), make closer contact with the quantum effects, which seem to lie, for the most part, outside its scope? It is just such a question which may lead one to suspect that the methodology of measurement will not always suffice as a birth-place for an adequate[1] formal theory. There may, however, be another answer, namely, that the Relativity theory *did not go far enough* in emphasizing methodological measurement concepts.

In order to illustrate this point, we shall give a somewhat lengthy quotation from one of Einstein's more popular[4] works:

[2] The simplified sketch given in this paper will be elaborated on the mathematical side in a paper to be presented for publication shortly, in one of the standard journals.

[3] The term "amorphous" (coordinate system) becomes clearer when compared with the "scale" coordinate system discussed below. Every point of the amorphous coordinate system is associated with some kind of idealized clock. All these clocks are synchronized by some more or less arbitrary prescription which in the field of visual coincidences involves the use of light beams.

[4] "Relativity," Henry Holt & Co., 1921, p. 5 et seq.

"Every description of the scene of an event or of the position of an object in space is based on the specification of the point on a rigid body (body of reference) with which that event or object coincides. This applies not only to scientific description, but also to everyday life. If I analyze the place specification 'Potsdamer Platz, Berlin,' I arrive at the following result. The earth is the rigid body to which the specification of place refers; 'Potsdamer Platz, Berlin,' is a well defined point, to which a name has been assigned, and with which the event coincides in space.

"This primitive method of place specification deals only with places on the surface of rigid bodies, and is independent on the existence of points on this surface which are distinguishable from each other. But we can free ourselves from both of these limitations without altering the nature of our specification of position. If, for instance, a cloud is hovering over Potsdamer Platz, then we can determine its position relative to the surface of the earth by erecting a pole perpendicularly on the Platz, so that it reaches the cloud. The length of the pole measured with the standard measuring rod, combined with the specification of the position of the foot of the pole, supplies us with a complete place specification. On the basis of this illustration, we are able to see the manner in which a refinement of the conception of position has been developed.

"(a) We imagine the rigid body, to which the place specification is referred, supplemented in such a manner that the object whose position we require is reached by the completed rigid body.

"(b) In locating the position of the object, we make use of a number (here the length of the pole measured with the measuring rod) instead of designated points of reference.

"(c) We speak of the height of the cloud even when the pole which reaches the cloud has not been erected. By means of optical observations of the cloud from different positions on the ground, and taking into account the properties of the propagation of light, we determine the length of the pole we should have required in order to reach the cloud.

"From this consideration we see that it will be advantageous[1] if, in the description of position, it should be possible by means of numerical measures *to make ourselves independent of the existence of marked positions (possessing names) on the rigid body of reference.*[5] In the physics of

[5] The italics are our own.

measurement this is attained by the application of the Cartesian system of coordinates."[6]

Now if we "make ourselves independent of the existence of marked positions (possessing names) on the rigid body of reference," we eliminate from our methodology just those characteristics by means of which the properties of gross matter (as such) are disclosed to the experimental physicist.[7]

The quantum effects, from a purely experimental point of view arise from the emphasis on matter as such. For example, the *scaling* of the motion of a charged oil drop, is compared with the *scaling* of electric potential differences, from which the physical existence of the election is *inferred*; or the scaling of hydrogen spectrum lines is compared with the scaling of macroscopic properties of the gas itself, from which stable states of the atom are *inferred*; etc., etc. In contradistinction to this process, the scaling of a *single* distinguishing mark (which distinguishing may be identified with the concept of "mass-point") does not stress the relative positions of the group of distinguishing marks attached to gross matter. To particularize, in the field of spectroscopy, one of the problems is to relate the relative positions of the spectrum lines, which are an array of distinguishing marks to the scale properties of the gross matter which is called the source of these lines. Thus it appears that from a codification point of view we must relate the array of ordered marks (the spectrum lines), to another array of ordered marks in the immediate neighborhood of the so-called "source of energy," namely the scaled current readings, the scaled gram-atomic weight, etc.

Thus, in making ourselves "independent of the existence of marked positions (possessing names),"[8] while we may find it "advantageous"[1] to employ the Cartesian amorphous coördinate

[6] We should say "*amorphous* system of coordinates" on account of the obliteration of distinguishing marks.

[7] No finite body is free of distinguishing marks. A finite body, by postulation has *boundaries* and these boundaries are its distinguishing marks. For example, *within* a gas such as hydrogen, under constant temperature and pressure, there are no macroscopic distinguishing marks. The distinguishing mark system of such a matter configuration is formed by its boundaries; that is, for example, by the contours of the glass tube which contains the hydrogen.

[8] As we shall see, these "names" become for us *ordinal numbers*.

system, nevertheless we have made a departure from the methodology of measurement. We have by stressing the "application of the Cartesian system of coördinates" eliminated the measured properties of matter[9] (as such), and so the employment of an amorphous coördinate system automatically gives us a "field" theory. Let us see what happens when these arrays of distinguishing marks are emphasized.

Remark On Ensuing Definitions. The definitions of *Virtual Contact, Direct Contact, Scale,* the *Proto-scale* and the *Coincidence,* are to be taken as tentative, in the sense that in application the meaning of the terms become clearer. After all, we are making marks on a piece of paper, which marks are intended to stimulate certain emotional responses. We are faced with the intricate problem of *communication.* In an attempt to convey a set of concepts, we might spend much thought and time on phraseology. If we do the definitions become bulky, that is to say, we go on making more marks on paper, and so expose ourselves to the demand for defining more symbols, etc. There ought to be an optimum number of familiar symbols for a working definition.[1] Following Bridgman, we shall (for the most part) let the definitions define themselves, in operation.[10]

Virtual Contact. When by some prescribed objective criterion we may establish the fact that a beam of light energy or sound energy (or other agency capable of affecting one or more of the senses) connects two distinguishing marks, then, these distinguishing marks are said to be in virtual contact.

Direct Contact. When the objective criterion for resolution of two distinguishing marks is not satisfied, the two marks may be said to be in direct contact. In the field of visual coincidences,[11] this criterion has to do with the overlapping of visible electromagnetic fringes.

The Scale and the Proto-Scale. A (physical) scale is a finite

[9] It must be emphasized that the *measured* properties of matter are revealed by comparing the distinguishing marks on matter to a set of *ordered* distinguishing marks on another piece of matter, called a scale (discussed in the next section).

[10] "The Logic of Modern Physics," P. W. Bridgman—The Macmillan Co., New York, 1928. This essay, of remarkable lucidity and insight, we shall refer to again.

[11] For the orientation of the term "visual coincidences" see "The Spacetime Scale As A Quantum Coordinate System," Jour. Frank. Inst. Vol. 216, No. 2, August, 1933.

assemblage of *ordered* and continuously identifiable distinguishing marks in virtual or direct contact with gross matter capable of interacting with one (or more) of the sensory organs of an observer, by way of some agency, such as light or sound energy. The *rational integers* (a sub-class of algebraic integers), namely, $\ldots, -3, -2, -1, 0, +1, +2, +3, \ldots$, are employed for purposes of ordering these distinguishing marks. We have, therefore, "marked positions, possessing names." A proto-scale is a finite assemblage of distinguishing marks in direct or virtual contact with gross matter. When the marks of proto-scale are *ordered* by the assignment of rational integers, one for each mark, the proto-scale becomes a scale.

Examples of Scales. An ordinary foot-rule, marked off, say in sixteenth inches is a scale; so is a meter bar, marked off in say millimeters. A marked comparator bar used frequently to measure distances between the photographic record of spectrum lines is a good example. The ordered distinguishing marks on the glass stem of a thermometer; the ordered marks on the stationary part of a voltmeter or ammeter are scales. These distinguishing marks are in direct contact with gross matter. Some of these scales are uniform, some non-uniform.

Do scales exist in which the distinguishing marks are in virtual contact with matter? To answer this question, consider a small glass tube, fitted with metal electrodes, and filled with mercury vapor. When a suitable electromotive force is applied to these terminals, the mercury vapor will begin to glow. The eye of the observer, whose retina is in virtual contact with the glowing vapor, notices no particular quantum effect. However, with a suitable arrangement of lenses and slits, a concave grating and photographic plate (which last may be replaced by the eye of an observer over the visible range) sharp lines, may be brought into focus on the photographic plate giving us a sharp line spectrum of mercury vapor. This assemblage of sharp lines forming an array of distinguishing marks, in virtual contact with the mercury vapor, is a proto-scale. When these lines are ordered by a comparator scale, they form of themselves a mercury scale.[12]

[12] In the paper of reference 11, these spectra are called spacetime scales. Now we shall postulate here that: All scales are essentially spacetime scales, for in order to compare

The Coincidence Defined. A pure coincidence consists in such a juxtaposition of a proto-scale mark and scale mark, that the observer asserts that the two marks are in direct contact. This assertion obviously depends in the nature of the resolving power criterion. In the field of visual coincidences[11] such criteria have been explicitly formulated. Here the retina of the eye is in virtual contact with the two marks in question. In the field of aural coincidences[11] these criteria have not been well formulated. Here, too the sensory mechanism is in virtual contact with two sounding marks, the agency of contact being sound energy. In the domain of tangible coincidences, where say, a finger tip is used to record a coincidence the sensory organ is (in more or less) direct contact with the coincidence. So far as the writer is aware, the question of resolving power has not been formulated for a tangible coincidence. Optical and aural aids to resolving power are in current use. In the field of tangible coincidences an aid to resolving power would imply a continuous run of gross matter from the two distinguishing marks in question to the finger tip. Now we shall attempt to show that all the types of coincidences mentioned here have one and the same formal codification.

Before doing this however we are faced with the general situation of the proto-scale distinguishing mark standing *somewhere* (and *somewhen*) between two scale distinguishing marks. When such a situation obtains the observer resorts to a process known as estimating. Now a single estimate involves the arbi-

---

two (or more) coincidences, we must postulate *simultaneity* of record. Thus we are obliged to think of a comparator scale as having at each mark, a *physical* clock, which clocks must be synchronized according to some prescription which involves the agency which connects them. In the field of visual coincidences this agency is the light beam. Now a uniform scale may be called a galilean[11] scale. It is with such uniform scales that we may, on last analysis, measure the macroscopic properties of mercury, such as its elastic, electric, magnetic, and thermodynamic properties. From this point of view it would appear that the quantum problem, at least, insofar as spectroscopy is concerned, involves a set of transformation equations which relate the ordering of spectrum lines (or Laué spots) by a galilean scale, to the ordering of the distinguishing marks in closer contact with gross matter (from which we codify its macroscopic properties) also with galilean scales. The quantum problem from this point of view thus appears to consist in a more or less deductive establishment of transformation equations between virtual contact scale systems, and direct contact scale systems.

trary assignment of a *proper fractional* number to the position of the proto-scale mark which expresses the observers judgment of the location of the proto-scale mark with reference to one of the two scale marks which lie in either side of it. Let us call this estimate, θ_m (m being a whole number) where

$$|\theta_m| < 1 \qquad (1)$$

If by some objective criterion we can declare the position of a proto-scale distinguishing mark, between two scale marks, stationary in spacetime, we may make many estimates of this stationary condition. Let us assume, with Bernouilli, that some statistical mean of m empirical estimates approaches a limiting value as $m \to \infty$. Thus if θ is the arithmetic mean (for example)

$$\lim_{m\to\infty} \frac{\theta_1 + \theta_2 + \ldots + \theta_m}{m} = \theta$$

In the symbolism which follows we shall think of θ as this arithmetic mean.[13]

The Scale Coördinate. Consider a meter bar scale marked off into one-thousand equal intervals. Then the number of millimeters per scale mark interval (hereinafter, s. m. i. ≡ scale mark interval) is one. The number of centimeters per s. m. i. is 0.1, and the number of meters per s. m. i. is 0.001. Suppose now, we wish to measure a length, i.e., the interval between two distinguishing marks of a proto-scale. We juxtapose our scale to our proto-scale, and arbitrarily bringing our scale origin mark (named zero) to the first mark of the proto-scale, we let the second mark of the proto-scale fall where it must. From the point of view of the scale concept we would record the reading as:

$$256\left[0.1\,\frac{cm.}{s.m.i.}\right] + 0.5\left[0.1\,\frac{cm.}{s.m.i.}\right]$$

Now, 256 is the ordinal number of the last scale mark beyond which we must estimate, while 0.5 is the estimate between the 256th and 257th scale mark.

[13] In the procedure given here we have glossed over the whole troublesome question between the relationships of a priori to a posteriori probability. For a clear cut account of the problem involved see, e.g., "Probability and Its Engineering Uses," T. C. Fry, D. Van Nostrand Co., New York, 1928. Chapter IV.

We may generalize this procedure by letting n be the generic symbol for the last scale mark involved in the measurement and Θ the statistical mean of the several estimates in the sense discussed above. Furthermore, let B be the number of units of length per s. m. i. In these symbols we may say that the length expressed in the amorphous coördinate system, ζ, is related to the length expressed in the scale coördinate system thus:

$$\begin{aligned} \xi &= n \cdot B + \theta \cdot B \\ &= B(n + \theta) \end{aligned} \tag{2}$$

Now a coördinate of the form: $B(n + \Theta)$ has a twofold aspect:

(a) The coincidence recording aspect which includes,
  (i) The operation of counting scale marks
  (ii) The operation of estimating between the *nth* and $(n + 1)st$ scale mark.

(b) The dimensional aspect symbolized by B.

We note first that B is a quantum relative to its own scale. The objective criteria of operation and configuration with respect to apparatus, by means of which we can distinguish between *a* (for example) magnetostatic charge and an electrostatic charge, are just the criteria for the assignment of the proper dimensionality to B. The dimensions of B, need not necessarily be those of length even in a three way euclidean space.

While in rectangular cartesian coördinates all the $B's$ are dimensionally homogeneous, in for example,

$$\left.\begin{aligned} x &= B_x(n_x + \theta_x) \\ y &= B_y(n_y + \theta_y) \\ z &= B_z(n_z + \theta_z) \end{aligned}\right\} \tag{3}$$

in spherical polar coördinates, we have

$$\left.\begin{aligned} r &= B_r(n_r + \theta_r) \\ \theta &= B_\theta(n_\theta + \theta_\theta) \\ \phi &= B_\phi(n_\phi + \theta_\phi) \end{aligned}\right\} \tag{4}$$

in which latter B_θ and B_ϕ may have the dimensions of say radians per s. m. i. on an angle scale. The right hand sides of (3) and (4) imply the physical existence of interlocking marked measuring

rods, for (3) a rectangular space net-work, and for (4) an interlocking system of radial rods, through which pass circular scales in latitude and longitude. Any matter configuration measured by such scale systems emphasizes, therefore, at the same time, the property of the body measured (proto-scale), as well as the property of the scale employed.

If we divide a piece of apparatus into two parts, one which includes only the scales involved, the other which includes that part of the apparatus adjunct to the scale, we may look upon apparatus as a configuration of

(i) The scale system
(ii) The dimensional system

By the term dimensional system we are to understand those physical inertial entities of apparatus which do not form a part of the scale system. For example, in an electrometer, the moving plates, the torsion fibre, the shielding case, the sulphur insulators, etc., are parts of the dimensional system.[14] In the terminology of scale systems and dimensional systems we may assert that the two-fold aspect of the scale coördinate arises through the fact that, $(n + \theta)$ emphasizes the operation of counting scale marks *common* to all measurement with apparatus, while B emphasizes the *differences* in operation and configuration between the several experimental dispositions.

In the case of a linear scale which considered by itself is a piece of apparatus where the dimensionality is that of length. "That part of the apparatus adjunct to the scale" is a part of the scale itself. We may infer from this that the length concept will hold a key position in the theory of measurement, and it is in fact this key position which allows of transformations between (e.g.) electrodynamic dimensional systems and thermodynamic dimensional systems.[2]

PART II. FUNCTIONALLY INTERCONNECTED DIMENSIONAL SYSTEMS

The Amorphous Coördinate and Its Want of an Explicit Dimensionality Symbol. Throughout much of the literature of Mathe-

[14] Even though not all of these parts may be necessary to define an electrometer for purposes of an operational-configurational[16] physical geometry.

matical Physics one finds the lack of *explicit* dimensionality in the amorphous coördinate one of its most serious shortcomings. Various devices for circumventing this desideratum have been given, and in concept, at least, some of these parallel very closely the conceptual background behind a coördinate of the form $B\ (n + \Theta)$. In discussing the rôle of mathematics in Physics, Bridgman remarks:[15] What we should like is some development of mathematics by which the equations could be made to cease to have meaning outside the range of numerical magnitude in which the physical concepts themselves have meaning. In other words, the problem is to make our equations correspond more closely to the physical experience back of them. . . . There is another aspect of the use of mathematics in describing nature that is often lost sight of, namely, that any system of equations can contain only a very small part of the actual physical situation; there is behind the equations an enormous descriptive background through which the equations make connection with nature. This background includes a description of all the physical operations by which the data are obtained which enter the equation. . . . The descriptive background gets its meaning only in terms of certain physical operations.[16]

An Example of Functionally Related Dimensional Systems (Causality). Now, it has been remarked that B, in $B(n + \Theta)$ need not necessarily have the dimensions of length per s. m. i. It may in fact be so many units of pressure per s. m. i., or volume per s. m. i., or temperature per s. m. i., or ergs per centimeter per s. m. i., or volts per s. m. i., or etc.

[15] Reference 10, p. 63.

[16] We have added to the term operational the term *configurational* throughout this paper. We have not space to go into the reason for this additional term. While Bridgman does not specifically use the configurational aspects of apparatus in the determination of dimensionality, the spirit of the essay appears to demand it. It is probably true that on last analysis the operational point of view will obtain. Nevertheless for a physical geometry, what does it matter whether two geometrically congruent parallel plate condensers (serving as part of an electrometer) are made on a lathe or by hand. The point is that a whole host of operations may be grouped under a particular class of geometric *configurations* of matter. Once the particular configuration is fixed the number of physical operations which determine dimensionality are enormously reduced.[2]

Keeping Bridgman's remarks in mind consider the apparatus which is used to establish the law of a perfect gas, namely,

$$\frac{PV}{\mathcal{T}} = R, \text{ a physical constant.} \tag{5}$$

In view of the generic relation (2) we may write

$$\left.\begin{aligned} P &= B_P(n_P + \theta_P) \\ V &= B_V(n_V + \theta_V) \\ \mathcal{T} &= B_T(n_T + \theta_T) \end{aligned}\right\} \tag{6}$$

where B_p is the number of units of pressure per *s. m. i.* on the pressure scale, etc. Transforming (5) by means of (6) we may write,

$$\frac{B_P B_V}{B_T} \cdot \frac{(n_P + \theta_P)(n_V + \theta_V)}{(n_T + \theta_T)} = R, \text{ a physical constant.} \tag{7}$$

Now as between two (or more) states of the gas for the *same* observer we may write,

$$\frac{B_P B_V}{B_T} \cdot \frac{(n_P' + \theta_P')(n_V' + \theta_V')}{(n_T' + \theta_T')} = \frac{B_P B_V}{B_T} \frac{(n_P'' + \theta_P'')(n_V'' + \theta_V'')}{(n_T'' + \theta_T'')} = \cdots = R$$

$$\frac{(n_P' + \theta_P')(n_V' + \theta_V')}{(n_T' + \theta_T')} = \frac{(n_P'' + \theta_P'')(n_V'' + \theta_V'')}{(n_T'' + \theta_T'')}, \text{ a pure numeric.} \tag{8}$$

Notice that (8) is the essence of the *law* of perfect gases, and of itself "ceases to have meaning outside the range of numerical magnitude in which the physical concepts themselves have meaning."

As for the calculus of the *B's*, this is a "relativistic" matter as Bridgman points out, and involves an invariance of operation and a spacetime transformation of configuration.

In this type of analysis the form of the law may be identical for the interaction between point electrostatic and point electromagnetic charges, but the subscripts attached to the *n's* keep the two laws distinct. However, these subscripts are attached *by virtue of the B's*, so that functions of $(n + \Theta)$ alone are from a dimensional analysis point of view in the same class as the amorphous symbols, P, V, or $\mathcal{T}$. If the *B's* are retained, the

equations must always express dimensionality explicitly, and automatically offer checks on the analysis, for, a hybrid expression like $B_T\ (n_V + \Theta_V)$ has no physical meaning. We may now answer the question: Under what condition are two dimensional systems functionally related? To particularize, consider two thermometric substances enclosed in an adiabatic envelope. They form dimensional systems. We notice that one shrinks while the other expands. Are they functionally related? If by the application of scales, experience shows that the scale readings are functionally related in the mathematical sense then the dimensional systems are functionally related in the physical sense. If, conversely, no mathematical functionality can be found, then the cause-effect relation may be denied. This pronouncement is in the nature of a cause-effect criterion, for physical science, and as a criterion has that degree of arbitrariness which only usage can justify.

PART III. THE FINITE DIFFERENCE IN SCALE COÖRDINATES

Minimum Change In The Scale Coördinate. From the point of view of the scale what shall we take as the minimum change in the scale coördinate. We notice that the smallest change we can detect without resorting to estimates is *one* relative scale quantum, i.e., to say,

$$n \to n + 1 \qquad \text{or} \qquad n \to n - 1 \tag{9}$$

Corresponding to this change the estimate changes from, say, Θ_1 to Θ_2.

For purposes of *computation* we shall postulate that $\Theta_1 = \Theta_2$ so that the change in the independent variable follows the rules:

$$\Delta n = \pm 1 \tag{10}$$

$$\Delta \theta = 0 \tag{11}$$

Example of A Difference Equation In Scale Coördinates. In view of (10) and (11) consider the equation of the parabola in amorphous coördinates:

$$y = x^2 \tag{12}$$

If this parabola is referred to a two-dimensional flat network we have:

$$y = [B(n + \theta)]^2 \tag{13}$$

Now if Δy is the change in y for a minimum change in the independent variable, we have, in view of (10) and (11), taking $\Delta n = +1$,

$$\begin{aligned} \Delta y &= B^2[(n + 1 + \theta)^2 - (n + \theta)^2] \\ &= 2B^2\left[\left(n + \frac{1}{2}\right) + \theta\right] \end{aligned} \tag{14}$$

which gives the change in y when $n \to n + 1$.

Consider the potential energy stored up in an elastically deformable body obeying Hooke's law:

$$V = \frac{1}{2} kx^2$$

where x is the strain which the body must support to acquire the potential energy V; and k is the constant stress per unit strain. In view of (14) we find

$$\Delta V = kB^2[(n + 1/2) + \theta] \tag{15}$$

We now raise the question: For what value or values of n do we have a position of stable equilibrium? Physically we require the position where the change in potential energy is a minimum.

Set, therefore,

$$\Delta V = 0$$

So that

$$n + \frac{1}{2} + \theta = 0 \tag{16}$$

Now under the conditions that

$$n \text{ is an integer} \tag{17}$$

$$|\theta| < 1 \tag{18}$$

both of which are intimately tied to the methodology of measurement with scales, we may readily verify that the solutions of (16) are uniquely:

$$n = 0, \theta = -1/2 \tag{19}$$

$$n = -1, \theta = +1/2 \tag{20}$$

So that in either case, $n + \Theta = -\frac{1}{2}$. Hence we obtain minimum change in energy when $n + \Theta = -\frac{1}{2} \rightarrow n + \Theta = +\frac{1}{2}$. This simple example shows the possibility of developing something in the nature of a selection principle from the methodology of measurement, and a physical principle lying close thereto.

PART IV. HEURISTIC GENERALIZATIONS

The Experience of The Pythagoreans. Consider a slab of matter in the form of a right triangle. By the application of scales it is found that as a result of many trials, the hypotenuse is related to the other sides thus:

$$B_s^2(n_s + \theta_s)^2 = B_x^2(n_x + \theta_x)^2 + B_y^2(n_y + \theta_y)^2 \tag{21}$$

When the triangle is so small, or alternately, when the scales are so coarse that the respective lengths are each less than the relative scale quanta (the B's) of the scale applied to them, we have the purely statistical relation:

$$B_s^2\theta_s^2 = B_x^2\theta_x^2 + B_y^2\theta_y^2 \tag{22}$$

Insistence on applying linear scales to curving figures necessitates the introduction of the concept of the infinitesimal measuring rod. For the infinitesimal triangle thus conceived we write in the notation of differential geometry

$$ds^2 = dx^2 + dy^2 \tag{23}$$

or

$$B_s^2 d\theta_s^2 = B_x^2 d\theta_x^2 + B_y^2 d\theta_y^2 \tag{24}$$

(21), (22), and (24) represent essentially three different types of codification schemes (although (21) includes the concepts involved in (22)). The formal connection between (21) and (22)

is fairly obvious. What, however, is the formal connection between (24) and (22)? Let us postulate that the connection is:

$$\left.\begin{aligned} B_s\theta_s &= B_s\int_0^{\theta} d\theta_s \\ B_x\theta_x &= B_x\int_0^{\theta_x} d\theta_x \\ B_y\theta_y &= B_y\int_0^{\theta_y} d\theta_y \end{aligned}\right\} \tag{25}$$

so that (21) becomes:

$$B_s^2\left(n_s+\int^{\theta_s} d\theta_s\right)^2 = B_x^2\left(n_x+\int_0^{\theta_x} d\theta_x\right)^2 + B_y^2\left(n_y+\int_0^{\theta_y} d\theta_y\right)^2 \tag{26}$$

The Experience of the Relativitists. Einstein and his followers have looked upon the measurable universe as being essentially Pythagorean in the sense that the infinitesimal quadratic form is a proper codifying scheme. The geometry of Riemann furnishes the necessary tools. In this geometry interval is defined by

$$ds^2 = g_{ij}dx^i dx^j,\ i,j = 1,\ldots,m \tag{27}$$

for an m-dimensional manifold. In this scheme of codification the variational principle

$$\delta\int ds = 0 \tag{28}$$

gives a geodesic which from the point of view of physics gives the dynamical accounting of a mass point singularity in a space which is characterized by the g_{ij}'s in (27).

Now analogous to (27) we may write in scale coördinates

$$B_s^2\left(n_s+\int_0^{\theta_s} d\theta_s\right)^2 = g_{ij}\cdot B^i\left(n^i+\int_0^{\theta^i} d\theta^i\right)\cdot B^j\left(n^j+\int_0^{\theta^j} d\theta^j\right), \tag{29}$$

$$i,j = 1,\ldots,m$$

for an m-dimensional manifold in which B may be interpreted as an absolute kinematic quantum,[11] n, an absolute quantum number; and

$$\theta = \int_0^{\theta_s} d\theta_s$$

a generic symbol for the statistical amorphous coördinate. g_{ij} are those macroscopic scaled properties which characterize the nature of the space we are working in. Now in a network it seems natural to take as a variational principle that

$$\delta\left[B_s\left(n_s+\int_0^{\theta_s}d\theta_s\right)\right]=\pm B$$

or that

$$B_s\,\delta n_s+B_s\,\delta\int_0^{\theta_s}d\theta_s=\pm B \qquad (30)$$

$+B$ for a minimal path, $-B$ for a maximal path. (30) implies that, since $\delta n_s = \pm 1$,

$$B_s\,\delta\int_0^{\theta_s}d\theta_s=0$$

or

$$\delta\int_0^{B_s\theta_s}B_s d\theta_s=0$$

or

$$\delta\int_0^{s}ds=0$$

which is the same condition as (28) provided the operator, δ, is defined, in part, by the operand which follows it. Hence the metric given by (29) develops among other things the Riemanman formulation of the Relativistic problem. Now these considerations though purely formal are suggestive of a type of geometry that might have some bearing on quantum phenomena. The quantum aspects seem to appear in n and B, and the classical aspects of physical law through the statistical mean Θ_s.

CONCLUSION

It appears to the writer that a particular methodology of measurement carries with it just those analytic elements which are appropriate[1] for the codification of data arising from this

methodology. If this conclusion is warranted,[17] then it would appear that a major problem facing mathematical physicists lies in the discovery of:

(a) The proper analytic elements for a given methodology.
(b) The formal relations between the sets of analytic elements arising out of these methodologies.

Ryerson Physical Laboratory,
University of Chicago

[17] Not so much from the subject matter of this paper as the spirit underlying the procedure.

Beyond Mechanism and Vitalism

BY

EDGAR A. SINGER, JR.

DURING the course of the last century it has grown increasingly clear that not all the issues with which an experimental science can be faced are experimental issues. If there were no other ground for this belief, history itself would force upon us some such conviction. For there are differences of opinion dividing men today that have divided men from the earliest times recorded, and in every one of the ages in between the self-same issue will have involved in dissension, not an occasional disputant here and there, but all the best thinkers in all the known sciences of their day. Yet at no time has either party to such strifes been in possession of facts ignored by the other; for had this been the case, the ignorant party must ere now have been driven from the field by force of the facts it ignored. It is fair to suppose that an issue which has come no nearer to solution as century after century added its contribution to the store of human knowledge, cannot depend for its decision on any information to be looked for from the centuries to come.

But if the matter of dispute in such interminable strifes is not, never has been, never can be one whose decision turns on a question of fact, on what can the decision turn? It would be easy to answer that in all cases in which an issue has proven itself perennial, the matter of difference is not indeed one of fact, but of conception. And all history would seem to confirm this opinion, for throughout its length, the speech of either party to such

enduring discords is witness to the fact that to each the conceptions of the other were inconceivable, the thoughts of the other unthinkable. The only trouble with this answer would be that it raised a question more difficult to resolve than any it pretended to answer. *Why* should it be impossible for one intelligence to grasp the world of another, both having before them the same range of facts?

In answer to this question, we are generally ready to suppose that where the conceptions of one man are inconceivable to another, the thoughts of one unthinkable to another, the reason must lie either in the different psychologies or in the different logics distinguishing the thinkers. But how confirm this supposition, where the differences are profound, all-dividing, long enduring? It is easy to find examples drawn from the minor experiences of life of interpretations of "fact" varying with the apperceptive background of the interpretor; it is common to find the range of "logical possibilities" open to a mind, varying with the mind's power of logical analysis. It is rare indeed to find the whole of humanity psychologically divided on a fundamental principle of world-interpretation, still more rare to find logical possibilities closed to the centuries opening at the suggestion of a late, more thoughtful day.

The present paper would like to consider the story of a quarrel of some 23 centuries' duration, throughout nearly all of which the disputants accepted a common logic and were differentiated only as to their psychologies; that is to say, were determined to their respective sides in the quarrel by motives of temperament, education, human interest and the like. Throughout these centuries, the psychology of the thinker alone could account for his acceptance of one and rejection of the other of two contending theories equally supported by fact. Then, at the end of these millenia, namely about 150 years ago, our story would have to tell of the sudden appearance of a third party to the quarrel, one whose difference from its predecessors lay not in its possession of a new and unprecedented psychology, but in its willingness to venture a new application of logic.

A quarrel of this duration will naturally have been called by

many names in the course of the centuries. We shall call it through the older and longer part of its history the Mechanist-Vitalist conflict, and through the later and briefer part, the Mechanist-Vitalist-Naturalist controversy. But the name is of no importance, so long as its connotation is clear. By whatever names the three schools be called, the logical differences that divide them may be most accurately defined, the history of their development most briefly summarized, and the psychology of this development most readily grasped, if we begin by setting down three propositions to which, for obvious reasons, we give historic titles; namely,

The Democritean postulate. Everything in Nature is structural in nature.

The Aristotelian postulate. Some things in Nature are non-structural in nature.

The Classic Logician's postulate. Nothing non-structural in nature is structural in nature.

Considering these propositions first from the point of view of pure logic, the formal logician of any school and any time would confirm the following observations respecting them:

First, each of the three pairs into which this triad of propositions falls constitute the premises of a formally valid syllogism.

Second, the syllogism of which any two of the three propositions are premises will have for its conclusion the formal contradictory of the third.

Third, as corollary of the first and second observations, no science could hold simultaneously all three of these "fundamental postulates," as we may conveniently call them throughout the sequel.

The importance of these observations may not at once appear, but what must at once be reassuring is the confidence with which one may assert their truth: it is a truth not likely to be disputed by any school of science belonging to any age. Perhaps this last statement is too self-evident to need elucidation, yet no one is likely to object to having before his eye an ocular demonstration of the formal rigor to which these observations pretend.

To provide this demonstration, no more is needed than to

construct the three syllogisms on which the argument depends, together with the contradictory of each conclusion. The array, which may be trusted to speak for itself, will be as follows:

(i) L. postulate. Nothing non-structural in nature is structural in nature.
D. postulate. Everything in Nature is structural in nature.
Conclusion. Nothing in Nature is non-structural in nature.
Contr. of C. Something in Nature is non-structural in nature. (= the A. postulate.)

(ii) L. postulate. Nothing non-structural in nature is structural in nature.
A. postulate. Some things in Nature are non-structural in nature.
Conclusion. Some things in Nature are not structural in nature.
Contr. of C. Everything in Nature is structural in nature. (= the D. postulate.)

(iii) D. postulate. Everything in Nature is structural in nature.
A. postulate. Some things in Nature are non-structural in nature.
Conclusion. Some things non-structural in nature are structural in nature.
Contr. of C. Nothing non-structural in nature is structural in nature. (= the Classic Logician's postulate.)

Nothing excuses the formality of this display of simple matters of logic unless it be the important part these syllogisms will be found to play in the solution of a most important problem; namely, the exhaustive classification of all possible opinions respecting the nature of Nature. Certainly the modern logician accustomed to express himself algebraically would condense all that has cost the scholastic logic so many words to unfold into one brief implication. Denoting the postulates by the initials (D, A, L) of the adjectives here used to distinguish them, and

letting the symbol *zero* (o) stand for a "null proposition" (i.e. a form of proposition false for all values of the terms), he would write, simply,

D *and* A *and* L *imply* o

Starting with this proposition of pure logic, he would make it a mere matter of routine to construct the array of all possible combinations of the postulates D, A, L and their contradictories D′, A′, L′, which do not imply o. Any one of these combinations would be a logically permissible opinion, but not all of these opinions would be of equal historic interest. The more important will be those based *on the minimum number of denials* (D′, A′, L′) *consistent with the number of their affirmations* (D, A, L). Systems fulfilling this condition may well be called *Constructive*, those that do not, *Skeptical*. Although skepticisms implying various degrees of negation occasionally appear in history, it is readily to be imagined that they exert no serious influence upon the development of empirical science. For such science must recognize each of our three fundamental postulates as formulating a demand either of experience or of logic; and that school of thought will best meet the requirements of experience and reflection which accommodates within itself the greatest number of empirical demands consistent with its understanding of logic. If then dismissing from our present consideration the relatively unimportant Skepticisms of history, we turn our attention to the systems called Constructive, we see that according as they fulfil the defining condition in one or another of all possible ways, they fall into three main classes:

(1) Attitude *undefined*, affirming no one of our three postulates and denying none;

(2) Attitude *ill-defined*, affirming one postulate and denying none;

(3) Attitude *well-defined*, affirming two postulates and therefore forced by logic to deny the third.

All possible schools of the Constructive class, are presented under names that seem appropriate to each, in the following table:

Attitude	School	Postulate Democritean	Postulate Aristotelian	Postulate Logician's
Undefined	Mystic	——	——	——
Ill-defined	Democritean	Affirms	——	——
	Aristotelian	——	Affirms	——
	Classic Logician	——	——	Affirms
Well-defined	Mechanist	Affirms	(Denies)	Affirms
	Vitalist	(Denies)	Affirms	Affirms
	Naturalist	Affirms	Affirms	(Denies)

In this table, blank spaces indicate that the school in whose row they appear neither denies nor affirms the postulate in whose column they appear; parentheses enclosing *Denies* indicate that the affirmations in whose row they appear logically imply the denial of the postulate in whose column they appear. With this understanding, the table furnishes a complete definition of each school named, and the name will be used in the sequel in no other sense than the sense so defined.

In a domain controlled by empirical postulates, there is great advantage in thus effecting an exhaustive display of the logical possibilities in the way of self-consistent but mutually contradictory opinions. The preliminary work of the logician reduces to a minimum the subsequent labors of historian and psychologist. This is particularly evident where, as in the present case, long periods of history are exhausted by the conflict of a limited number of schools, whereafter there suddenly appears a new doctrine making its claim for a place in the sun.

Thus in the present instance, the historian will have no trouble in verifying two brief statements summing up in a word the whole history of constructive natural philosophy:

I. Prior to the 18th Century every well-defined system of philosophy was either Mechanist or Vitalist. Neither then nor at any time could any self-consistent system be both Mechanist and Vitalist (as our table clearly shows).

II. At the end of the 18th Century we find the first system that proposes a natural philosophy consistent with neither Mechanist nor Vitalist thought. It lies beyond the two. This philosophy,

to give it a name, we have here called *Naturalist*, and its characteristic lies in its simultaneous affirmation of the Democritean and Aristotelian postulates, with the consequent denial of the Classic Logician's postulate. The proposal of this new philosophy comes from Immanuel Kant, first in his *Kritik der reinen Vernunft* and then in his *Kritik der Urtheilskraft*.

If the historian finds no difficulty in verifying the facts thus briefly summarized, the psychologist will find no more in explaining why the history of opinion should have developed in this way. It is easy to see why the order of experience usually called "physical" should seem to confirm the postulate of Democritus. It is easy to see why the order of experience usually called "biological" should seem to confirm the postulate of Aristotle. It is impossible not to see that these two empirically recommended principles cannot be held simultaneously, if the Logician's postulate is a sound principle of logic. But to all thought prior to Kant, and to most of the science of today, this Logician's postulate does seem a form of proposition (No non-x is x) as valid for all values of the terms as is the syllogism itself. Only a thought highly matured in reflection and deeply versed in its subtleties could have the courage to propose and the technique to justify the rejection of the Logician's postulate. This courage to propose, Kant certainly possessed; and if the technique to justify lay somewhat beyond the reach of his day, it is little wonder. Whether in the century and a half that has elapsed since Kant's time the development of logic itself may not have put the logician in possession of a technique equal to this difficult undertaking, is a question to be considered.

The post-Kantian development in technique that promises the modern logician the greatest advantage over the classic in applying his logic to experience is suggested by a happy term introduced by De Morgan, in his *Formal Logic* of 1845: the term, *universe of discourse*. The development is analogous to many another familiar to the historian of science, developments that have in one way or another made explicit an element of relativity latent in previous thought. Perhaps the earliest analogue one can think of is the transition from *the horizon*, to *my, your, his horizon*.

The analogy, at any rate, is striking; for what De Morgan points out is that certain class-relations formerly supposed to be definable for *all the universe*, are in fact definable only for *all universes*, i.e. for any one of the endless number of *universes* of which each includes "everything" pertinent to some present topic of discourse, and "nothing" impertinent thereto. A universe so conditioned may well be called *one's universe*, as distinguished from *the universe*; its content must vary with its context, much as the landscape within the horizon varies with the view-point. Within each and every *universe of discourse* the rules by which classic logic established such class-relations as *inclusion*, *non-inclusion*, *contradiction*, continue to hold; but the relations established within one universe cannot be assumed to hold within another. And thereby hangs the sequel of the tale so far told.

The correlation most importantly affected by De Morgan's principle of relativity, is the one to establish which the logician has always employed, and must continue to employ, the terms *everything* and *nothing:* it is the relation between two classes said to be *contradictory classes*. To define this relation between two classes x and non-x (e.g. *structural* and *non-structural*) the classic method was content to set down the double condition:

Everything is either x or non-x.

Nothing is both x and non-x.

The modern logician would put his conditions defining contradictory classes in a more conditional way:

If x and non-x are to be contradictory classes in any universe of discourse, then there must be found an empirically meaningful term, *u*, such that throughout a given discourse, the discutant proposes to abide by the two propositions:

Every *u* is either an *xu* or a *non-xu*.

No *u* is both an *xu* and a *non-xu*.

So long as the definer's discourse confines itself to the kinds of thing of which each is some kind of a *u*, and to the number of things of which each is one *u* of some kind, he is speaking within the same universe of discourse, and may refer to *every u* as "everything," to *no u* as "nothing," quite in the classic manner. But by putting his conditions in the modern form the logician

has warned himself against a certain danger. Namely this: Let a certain u, x, non-x, be recognized as fulfilling the conditions defining a universe and two of its contradictory classes; and let a certain v, y, non-y, fulfil the analogous conditions resulting from substituting v for u, y for x, non-y for non-x. The modern does not permit himself to assume that he may substitute v for u, and still assert that (1) Every v is either x or non-x, and (2) No v is both x and non-x. He recognizes that in making the substitution he will have changed his universe of discourse, and that classes defined to be contradictories within one universe are not thereby established as contradictories within a second.

The trouble that must arise from not making the definition of contradictory classes relative to a given universe of discourse, is a matter of long experience. It had made itself felt well before Plato's time; and in one form or another it furnishes the problem faced by Plato throughout the run of his dialogues. Its common nature may be gathered from a single example wherein *the moral* and *the immoral* purport to have been erected into contradictory classes. But to have made of *the moral* and *the immoral* contradictory classes is to have affirmed as true throughout the world two propositions; namely,

Everything is either moral or immoral.

Nothing is both moral and immoral.

But, now, to accept the first proposition for all meanings of *thing*, the classic must be prepared to maintain (since a triangle certainly *something*) that

Every triangle is either moral or immoral.

And to accept the second proposition for all meanings of *thing*, the classic must be prepared to deny (since Themistocles was certainly not *nothing*) that

Themistocles was both moral (when adult) and immoral (when adolescent).

The classic mind, presented with difficulties of this order (they recur for all instances drawn from common speech of contradictory classes) is led to the most curious speculations as to what must be real, if *the real* is to be that of which one can hold self-consistent discourse. As for the modern, faced with this

particular "moral" problem, the deeper his thought, the greater his difficulty in fixing on any universe of discourse within which he would be willing to construct a *moral* and an *immoral* to satisfy at once the demands of logic and the needs of common use. But he would not regard the example cited nor any number of its kind as being final evidence that no such universe was to be found.

The situation just described has a practical consequence of interest to the sequel. In the case cited, and in all other cases in which a like difficulty of defining arises, your modern will be chary of using a certain principle sanctioned by classic use, the principle that permits the translation of negative judgments, No x is y, Some x is not y, into equivalent affirmative forms, All x is non-y, Some x is non-y. To be sure, the modern is as willing as the classic to admit the validity of such "translation by privative conception," wherever the universe of discourse is so well-established as to admit of no ambiguity in the meaning of the *thing* referred to in the conditions establishing *y* and *non-y* as contradictory terms. But his general caution in accepting as well-established the stability of any universe wherein matters of experience are the subjects of discourse, extends to every empirical application of the principle of privative conception. The importance to our present topic of this modern reserve in the presence of proposed translations is readily seen. It appears the moment one stops to consider whether the affirmative Aristotelian proposition, "Some things in Nature are non-structural in nature," is or is not under all conditions equivalent to the negative proposition, "Some things in Nature are not structural in nature." If it be, the Democritean and the Aristotelian propositions are formal contradictories; Mechanist and Vitalist are alike right in supposing the choice between the two to present to the logical mind a formal dilemma. If it be not, the compatibility of the Democritean and the Aristotelian motives in science is a possibility still open to discussion. On the outcome of this discussion depends the possibility of defining a self-consistent science that, accepting both the postulates of Democritus and of Aristotle, must of necessity deny the postulate of Classic Logic. So doing, this new conception must place itself in formal contradiction with

both the Mechanist and the Vitalist systems: it must present a third logical alternative "lying beyond the two." To the demonstration of the self-consistency of this third alternative—this "Naturalism" as we have called it—the remainder of the present discussion is devoted.

The demonstration to be convincing should proceed by definite steps; and three such steps suggest themselves at the outset as necessary and sufficient to our purpose: the first, establishing that universe of discourse within which *structural* and *non-structural* are to be defined as the adjectives of contradictory classes of "something;" the second, establishing that universe of discourse within which may be supposed to lie the *things* referred to by the Democritean "everything" and the Aristotelian "something;" the third, arriving at such an understanding of *Nature* as will leave the Naturalist's system of postulates not only logically permissible, but empirically useful. These three steps will be seen to have been so designed and ordered that each in turn should arrive at a definition suited to the needs of logic of some one of the three expressions our postulates leave in the vernacular: *in nature*, *thing*, *in Nature*.

(1) The universe of the *structural* and *non-structural in nature*. As has been said, to define *structural* and *non-structural* as adjectives of contradictory classes, one must set down the meaning of that *u* for which one is willing to accept as true the two defining conditions:

Every *u* is either a *structural* or a *non-structural u*.

No *u* is both a *structural* and a *non-structural u*.

Our proposal is that for this algebraic symbol *u* we write the word *class*. This can only mean that throughout our discourse, *structural* and *non-structural* shall remain adjectives qualifying classes, and so qualifying them that within a universe composed exclusively of classes (as distinguished from "individual things") the *structural* and the *non-structural* constitute contradictory *Classes of classes*. (For brevity and clarity, it will be convenient throughout the sequel to distinguish by the use of a capital the *Class* that is a "class of classes," from the *class* that is not a class of classes, but perhaps a "class of things.")

It is technically inelegant but may be practically helpful to offer at once some hint of what will ultimately be meant by contradictory Classes of classes. One will readily suppose that in all contexts the differentia distinguishing *two Classes* is to be sought in some difference distinguishing *two principles of classification* on which respectively the two classes will have been formed. The school-logician would sum the matter up by saying: Classes are to be differentiated in terms of their respective *fundamenta divisionis*. This kind of distinction between Classes is familiar to every one, and is to be illustrated wherever "cross classification" is used. As an example, the writer happens to recall an amusing exaggeration attributed to the late Professor Cope. Bent on impressing on his audience the essential difference between "artificial" and "natural" classifications of living forms, Cope was led to remark that "the trouble with artificial classification is that an animal an inch long and a mile wide may be the nearest relative to an animal a mile long and an inch wide." No doubt the company was prepared to admit that so far as its experience went, there would be no world in which two Classes of classes, the one formed on the basis of likeness and difference of shape, the other on the basis of likeness and difference of cousinship, were likely to have many classes in common. And this admission was all the biologist required to enforce his point. But had he then asked the company to formulate the general conditions under which two Classes of classes, not only *did* not in some given world, but *could* not in any imaginable world, have a single class in common, the company must, one thinks, have been greatly at a loss to answer. So at least should we have been; for at the end of this first step of our program, we are in position to say no more concerning the contradictory Classes to be composed the one of *structural*, the other of *non-structural* classes, than that it is our wish so to proceed in the further classification of "everything in Nature" that every class of such things shall fall into one or the other, and none into both of these Classes. Having laid this demand on ourselves, nothing is to be foreseen as to our ability to fulfil it; this cannot appear until we shall have completed the second step of our progress; i.e. until we shall have

arrived at some conclusion as to the universe of things to which Democritus' *Everything in Nature*, and Aristotle's *Some things in Nature* must alike refer.

(2) The universe of *everything* and *something* in Nature. In entering on this second step of our program, we may take immediate advantage of the results attained by the first. Whatever the *things* referred to by the subject-terms of the Democritean and Aristotelian postulates, the *things structural in nature* and *things non-structural in nature* appearing in the predicates are now taken to be *classes*; and, namely, such structural classes and such non-structural classes as constitute two mutually contradictory Classes of classes. Consequently, we are now in position to reformulate the two postulates in a wording more suitable than the original vernacular to the purposes of logical analysis. So restated these postulates become,

D. Everything in Nature is (included in) a structural class.

A. Some things in Nature are (included in) a non-structural class.

Now, since classes may be included in classes, nothing in this rewording prejudges the issue as to whether the subject-things of Democritus and Aristotle are or are not identical with the predicate-things of their postulates; namely *classes*. But any attempt to substitute the word *classes* for the word *things* of these postulates makes of them two contradictory judgments, and therewith precludes all possibility of holding them simultaneously true. We should have, namely, on making the substitution,

D. Every class in Nature is a structural class.

A. Some classes in Nature are non-structural classes.

But in a universe of classes wherein structural and non-structural classes have been made to constitute contradictory Classes of classes, a certain "translation by privative conception" is a formally valid process: in this universe, the proposition "Some classes are non-structural classes" is exactly equivalent to the proposition "Some classes are not structural classes." The contradiction between this, and the Democritean postulate is formal. In *no* logic could the two propositions be simultaneously

entertained. We must then recognize that if the two propositions are not to remain in formal contradiction, one thing the "things" they both refer to cannot be, is the thing called a *class*. But the one sort of thing whose definition implies that it cannot be a class, is the "individual thing." Therefore, the only universe of things for which the Democritean and the Aristotelian postulate can hold without becoming contradictory propositions is a universe of individual things. Does this mean a universe including every "individual thing" to which common discourse may have given a name; a universe including then the sensation of Thales on falling into that ditch, the emotion of Cellini on seeing his Perseus stand forth in bronze, the intention of Beethoven in writing the Fifth Symphony? It has already been said that any modern universe of discourse takes itself less seriously than the classic Universe; your modern assumes without compunction that however many the kinds of thing his universe may have taken in, it must have left many a kind out. Nor is it pertinent to our present undertaking that we so much as stop to enquire as to the broadest genus of empirical things for which the Democritean or the Aristotelian postulate might, in the light of experience, be taken to hold. We are not asking whether either proposition is true, but whether under any definable conditions the two can be held compatible. This much we might well enough determine by conducting the rest of the discussion in symbolic terms, terms e.g. that substituted for the *everything* of Democritus the expression, *every individual of the u-class*; and for the *something* of Aristotle, *some individual of the* (*same*) *u-class*. But to escape the irritation that results from mixing symbols with words, let us assign to the word *body* the momentary function of exactly replacing (the substantive part of) the two expressions indicated. Whether a natural science that went so far as to enquire into the truth of either postulate would be content to retain this term, or would be led, for the sake of preserving either in empirical truth, to suggest another term,—this in no wise concerns the present step of our argument. Making this substitution of *body* for *thing*, the postulates become

D. Every body in Nature is (a body) included in a structural class.

A. Some bodies in Nature are (bodies) included in a non-structural class.

To which premises the syllogistic conclusion is of course

C. Some bodies included in a non-structural class are (also) bodies included in a structural class.

At this point, nothing of our matter may be settled, but at least something begins to appear. If it were not for the formal contradiction established between the Classes to which the predicate terms of the Democritean and Aristotelian premises respectively belong; if, instead, these terms connoted no more than classes belonging to *different* Classes in some cross-classification, no one would see the slightest objection to admitting that one and the same body might simultaneously belong to a class of the one, and a class of the other system of classification. The 18th Century product facing me is an excellent time-piece; it is also a thing of beauty: the two classes can consistently hold this and many other *bodies* in common. But then nothing tells us that when all our terms shall have been defined *the useful* and *the beautiful* will be found to lie in contradictory Classes of classes; whereas this was the very condition by which the *structural* and *non-structural* were originally defined. In fact, we cannot so much as assume *the useful* and *the beautiful* to fall within mutually exclusive Classes; i.e. Classes that can have no class in common. Therefore, nothing we can infer from this or any like example of the common membership of classes included in different Classes can exclude a possibility which if it be an actuality must put an end to any hope of harmonizing the Democritean and Aristotelian demands. It is the possibility that only such classes may have members in common as fall within Classes having classes in common. From this it would of course follow that as the Class including *the structural*, and the Class including *the non-structural* can by definition have no class in common, they could have no individual members (bodies) in common. Democritus and Aristotle would be irreconcilable.

These reflections give us some idea of the difficulty of the Naturalist's undertaking. In themselves they do not prove it hopeless; they merely present possibilities not yet excluded and

presumably hard to exclude, such that if they were proven actualities *would* be fatal to the Naturalist's hopes of establishing a "third alternative." There are however certain modern logicians who would go far beyond the (perfectly true) comment that up to this point the Naturalist's case stood unproven. They would pretend to show from premises already accepted, and by methods of an accepted logic, that the Naturalist's ultimate thesis is not only as yet unproven, but can immediately be disproven. If right, their argument definitely reaffirms the Mechanist-Vitalist "dilemma" of the past.

The argument of these logicians can most readily be followed in the technical idiom of logic. To test the matter with the least waste of words, let us establish the following symbols:

(1) C and C′ = two contradictory Classes of classes.
(2) p = class included in C,
(3) q = class included in C′,
(4) b = body.

Now, Naturalism cannot hope to attain its purpose unless it is prepared to show that there can well enough be "in Nature" a body, b, such that

(i) b is included in p; and
(ii) b is included in q; and yet
(iii) b is not included in the null-class (O).

But, our logicians point out, if *is included in* is to remain a transitive relation, as classic and modern logic alike demand, the following propositions must hold:

(a) If b is included in p, and p is included in C, then b is included in C.

(b) If b is included in q, and q is included in C′, then b is included in C′.

(c) If b is included in C, and b is included in C′, then b is included in CC′.

(d) If b is included in CC′, and CC′ is included in null, then b is included in null.

Now the first premise of (d) is true by reason of (2) (3) (4). The second premise is true by reason of (1). Therefore the conclusion of (d) is true, and the proposition (iii) is contradicted.

But this telling argument to prove the ultimate hopelessness of the Naturalist's undertaking is not without answer: its answer lies implicit in our whole presentation of what is modern in logic; at least, so far as this modernity results from the recognition of relativity, i.e. of the variation of one's universe with the matter of one's discourse. For the modern as for the classic, *is included in* must indeed remain a transitive relation *so long as it remains any relation at all. But no relation at all either of inclusion or non-inclusion can be established between terms not of the same universe.* The same principle, one that must underlie the use of logic in all its applications to extralogical data, may be put in a form more available to practice; its import is fully conveyed in three propositions:

(1) If from any data in the form of logical inclusion, and from any reasoning confined to logical implication, it shall be proven that a body is included in the class of bodies pp′, or in the class of bodies qq′, *then that body is no body.*

(2) If under the same conditions it shall be proven that a class of anything is included in the Class CC′, *then that class is no class.*

(3) If under the same conditions it purport to have been proven that a body b is included in the Class of classes CC′, *then the reasoning is at fault.* No data presented in the form of logical inclusions, no inference observing the conditions of logical implication, can have established the inclusion of any individual in any Class of classes, neither then in C, nor in C′, nor yet, a fortiori, in C *and* C′.

The import of the last statement must not be misunderstood. It is not that the relation of inclusion cannot be established between individual and class, as it can between class and class: the famous bit of reasoning, If all men are mortal, and Caesar is a man, then Caesar is mortal, is sound reasoning because and only because it is a valid syllogism in *Barbara.* It is not, either, that logical inclusion is anywhere other than it is in this syllogism, a transitive relation. It is that between the things in one universe, and the things in another no logical relations can be established. Thus in a universe whose things are individual bodies, any one of these bodies may be included in a class of bodies, and that class

included in a second class of bodies, to the result that the first body will be included in the second class. Again, in a universe whose things are classes (and explicitly *not* individuals) any one of these classes may be included in a Class of classes, and that in a second Class of classes, to the result that the first class will be included in the second Class. In each case, we cling firmly to the transitivity of all the inclusions supposed. But now consider a third situation deceptively like yet essentially different from the two preceding. Surely, those modern logicians are right who find something wrong in the "syllogism," If *Caesar* is a man, and *man* is (the name of) a genus, then *Caesar* is (the name of) a genus. Only, these critics are hasty in supposing the ways of righting this wrong to be confined to the two they see; namely, either (1) we must assume one or the other premise (they decide on the first) to be not a logical inclusion, or (2) we must assume logical inclusion to be not a transitive relation. For in fact there is a third alternative. This solution appears the moment one resorts to the trick (it is our own, not theirs) of so italicising words as to show which are used to denote things named, and which are used as names (i.e. words) alone. The "man" of the first premise, and the "*man*" of the second do not belong to the same universe; and the "evidence" that transitivity does not hold, is confined to the observation that the "conclusion" is—not a false proposition—but no proposition at all. And if the "consequent" is no proposition, it is because the "antecedent" is a proposition (in the form of a conjunction of two propositions) from which no inference can be drawn save one of the following class; namely, all conjunctions of propositions of which each is inferable from *one alone* of the propositions conjoined in the antecedent. Evidently, the consequent of the pseudo-syllogism cited does not fall within this class. It is in fact an inference that could only be justified on the assumption that the two propositions conjoined in the antecedent possessed a common (*middle*) term. That they do not, is clear: of the terms mistaken for identical, "man" and "*man*," the first belongs to one universe, the second to another. But if no implication hold between the given antecedent and the proposed consequent, then from the "implication" supposed to

hold we can infer nothing at all,—not then the disjunction proposed by our critical logicians: "either an individual cannot be included in a class, or inclusion is not a transitive relation."

Our answer to these logicians "refutation" of Naturalism, however valid it may be, pretends to do no more than remove a certain obstacle that some have considered to be a fatal obstacle to the solution of a difficult problem. It has shown that there is nothing in the transitive nature of inclusion to justify the implication that if a body b be included both in a class p, and in a class q, of which the p-class be included in a C-Class, the q-class in a C′-class, then the body b must be included in a CC′-Class, that is to say, in a null-class. If the implication *did* hold, we should have to abandon at once any attempt to entertain the Democritean and the Aristotelian postulates as simultaneous conditions of any science of Nature. Naturalism, as a third alternative to the horns of that "dilemma" of which the Mechanist impales himself on the one, the Vitalist on the other, would have to be abandoned as a logical possibility. But to say that this third alternative does not yet have to be rejected, is not to say that it must—or even may—from now on be accepted. The fact is, as a system of thought, Naturalism is not yet sufficiently well-defined to enable us to judge of its availability to a philosophy of nature. It has shown why two classes included respectively in contradictory Classes of classes, need not themselves be contradictory classes of things. Such classes of things (bodies) may then for ought we can see possess some things in common. But then again (as was shown in a previous paragraph) for ought we yet know, they may not. The critical question remains as before: How establish the requirement that two Classes, C and C′, shall include some classes having some things in common, yet shall include no classes having all things in common; i.e. shall include no common class? How, in short, are contradictory Classes of classes, which classes are to include common members, to be established in a universe of things *found* in Nature; a universe, one would say, in which it was beyond the power of science to control the number of things that should or should not exist in any of its man-made classes? If this question can be answered

at all, it can only be answered after the "third step" announced in our initial program shall have been completed.

(3) The Universe of *Nature*. This third step, as was anticipated at the outset, must carry us on to a consideration of the phrase *in Nature*, the one bit of our postulates' original wording that has not yet been brought out of the vernacular into the well-defined idiom of science. The form this discussion of Nature should take, can only be made apparent after having first recalled the positions already arrived at, in an order to make quite evident the way that remains to be traveled. We sum up then as follows:

It has been assumed that

(1) Of a manifold of bodies, b, each one is included in at least one of a manifold of classes of bodies, c.

(2) Of the manifold of classes, c, each is included in one or the other of two Classes of classes, C or C′; and none is included in both, C and C′.

For convenience, we may divide the array of c-classes into the c-classes included in C, and those included in C′, calling the first p-classes, the second q-classes. Then we pretend to have shown that

(3) Though p be included in C, and q be included in C′, and though CC′ is a null-class, yet this does not imply that pq is a null-class.

The demonstration of this last non-implication depended, it will be remembered, on the logician's ability to distinguish two universes of discourse, the one of classes, the other of things. But this distinction of universes is of no interest to the experimental scientist, unless it can be shown that it is applicable to exactly the classes, and exactly the things to which the postulates of Democritus and of Aristotle alike refer. Now the *things* of these postulates purport to be not only *bodies*, but explicitly *bodies in Nature*; and the classes purport to be not only classes, but classes composed exclusively of these same bodies in Nature; classes, then, that may be called, *classes in Nature*. And this brings with it the reflection that, however the universes of bodies falling into classes, and of classes falling into Classes, may be kept distinct for logic, they must not be kept so distinct but that the

two universes of discourse may find simultaneous application to the one Universe of Nature. Hence two questions:

(1) How are the two universes to be kept distinct for logic, yet made available for simultaneous application to Nature?

(2) How are the two universes to be so applied to a classification of things in Nature, as to make these things fall into only such classes as will be found included in the contradictory Classes of structural and non-structural classes? In particular how may we establish in the idiom of logic, the conditions making it certain that nowhere in Nature shall the observer *find* a class of bodies simultaneously identified with a structural and in a non-structural class?

The answer to all these questions will, we think, be forthcoming if the answer to the last is clear. And our answer to that must be, "The thing cannot be done." That is, "in the idiom of logic alone" the conditions cannot be formulated which shall exclude from two Classes of classes, themselves logical contradictories, any common *class of things*, yet leave these classes free to possess *some things* in common. It may be, however, that what cannot be done in the idiom of logic alone, can be done in the idioms of logic and *arithmetic* combined. The experiment remains to be tried.

As the remainder of this paper is to be no longer critical but constructive, it may content itself with the fewest and briefest statements that will furnish another thinker with definite matter to criticize. The order in which these proposals are offered will, one hopes, sufficiently suggest the motives for making them:

(1) The capitalized *Nature* (as distinguished from *nature*) is a singular term and admits of no plural. (Cf. the Kantian sentence, *Es giebt nur eine Natur*; and the Machian, *Die Natur ist nur einmal da.*)

(2) *Bodies in Nature* are not *included* in Nature, as individuals are (logically) included in a class; they are *contained* in Nature as summanda are contained in (i.e. add up to) a sum. It is for this reason that any discussion of the classes formed of *bodies in Nature*, must require not only the idiom of logic, but also that of arithmetic. (By *arithmetic* we understand an algebra whose terms are numbers.)

(3) A *class in Nature* is any class of bodies of which at least one is a body in Nature.

(4) A *Class in Nature* is any Class of classes of which at least one is a class in Nature.

Consistently with our formal understanding that the problem with which we are faced can neither be set nor solved in the idiom of logic alone, we shall divide the problem-setting into two parts: the first, formulated in the idiom of logic alone; the second, in the idiom of logic and arithmetic, together.

(i) Conditions set by logic alone.

(1) The Class, C, of structural classes, and the Class, C′, of non-structural classes, are contradictory Classes of classes. This implies the condition

(2) CC′ *is included in* the null-class (O).

(3) (*p is included in* C) *implies* (p′ *is included in* C). This implies that the universe of individuals within which p and p′ are contradictory classes is itself a structural class.

(4) (q *is included in* C′) *implies* (q′ *is included in* C′). This implies that the universe of individuals within which q and q′ are contradictory classes is itself a non-structural class.

(ii) Condition set by logic and arithmetic together.

(5) *If* (p *is included in* C) *and* (q *is included in* C′) *then* ((pq is greater than o) *implies that* (*either* (pq′ is greater than o) *or* (p′q is greater than o))).[1]

In expanding this exact statement into the vernacular for ease of apprehension, one may admit the use of a few words which if not formally defined will be seen to be readily definable within the limits of logic and arithmetic. Our meaning then is this:

Required:

So to classify a numerable (finite or infinite) manifold of bodies whose total number is the number of bodies in Nature, that (1) every class of bodies shall be either a structural or non-structural class, (2) no class of bodies shall be both, (3) leaving it to experiment to determine whether or not there are any bodies common to a structural and a non-structural class.

[1] Here, the symbol o is the *zero* of arithmetic, not the *null* of logic. There is no confusion of symbols, for the relation-symbols of logic can always be kept distinct from the relation symbols of arithmetic. Thus, in the expression, x *is included in* o, the o is the *null* of logic; in the expression x = o, the o is the *zero* of arithmetic.

Condition necessary to solution:

If p be any structural class, and q any non-structural class of bodies, such that the number of bodies common to p and q is greater than zero, then the number of bodies included in p and q must either be less than the number included in p, or less than the number included in q.

Whether or not there is to be found in Nature a pq greater than zero, is an experimental question.

If the meaning of this step in the problem's solution is clear, then the difficulty experienced by an experimental scientist in meeting the condition it imposes on contradictory Classes in Nature is also clear. It is the difficulty implicit in the word *implies* of the more exact statement, in the corresponding word *must* of the more colloquial statement of the solution. An experimenter may *make* what descriptive classes he will, but he can only find, not make, the number of bodies in Nature falling within any class he may have designed. How then can he establish in Nature contradictory Classes of classes, by requiring that the number of things in Nature belonging to any class, not only are not, but shall not be equal to zero?

For the moment, our demonstration of the compatibility of the Democritean and Aristotelian postulates must rest on this question. It is one to which the author hopes to return on some future occasion. Meanwhile, he must content himself with referring to preliminary studies looking to its solution.[2] Suffice it for the present to express a personal opinion. If no difficulty more serious than the solution of this problem stand in the way of Naturalism, then this "third alternative," this philosophy "beyond Mechanism and Vitalism," reconciling as it does the two great historic demands arising respectively from the "structuralism" of Democritus, and the "functionalism" of Aristotle, is likely to be available for and likely best to serve the experimental science of the future.

Department of Philosophy
University of Pennsylvania
Philadelphia, Pa.

[2] *Proceedings World-Congress Philosophy*, Oxford 1930. "Logic, and the Relation of Mechanism to Function."

The Problem of Vital Organization

BY

RALPH S. LILLIE

IN CONSIDERING this problem a distinction should first be made between its scientific and it philosophical aspects. The scientific problem is that of defining in exact understandable terms those conditions and factors (with their various types of combination and interplay) which make possible the synthesis of the living organism from the simpler elements of the non-living environment, and also its maintenance in the adult state as a fully developed and autonomous organic individual. The problem as thus stated is one to be approached by methods of observation and experiment, leading to verifiable results which are then expressed in systematic and intelligible form. The final statements or representations thus reached, the accepted scientific generalizations, inevitably take on a theoretical form, and as such are necessarily partial and abstract. Many of them, in fact, are to be regarded primarily as simplifications or models devised in the interest of clear representation or understanding. Their relation to concrete reality may range from attempts at exact portraiture to formulæ or diagrams having little more than heuristic significance. Any realistic philosophy of nature must always remember how incomplete and provisional are the theoretical conceptions of any natural science.

There is also the philosophical method of approach which aims at defining the most general existential conditions—those inherent

in the constitution of an independently existing Nature—which must be presupposed as the necessary foundation or prerequisite for the vital development. The evolution of living organisms has occurred in nature; historically it appears as a process of ever-increasing complication. At once we encounter what seems (or has seemed) to be a dilemma. The evolutionary diversification of living beings, culminating (as it has done) in the appearance of human life on this planet, is a process which seems to run counter to those broad uniformities of large-scale action expressed in the laws of energy. These conditions, left to themselves, tend to *even out* differences of potential; the universe appears as "running down," as proceeding toward a simpler or less diversified (more "probable") state; the apparent trend, if not toward homogeneity or uniform distribution of components, is at least toward a final state in which all tensions balance and progressive change ceases. The vital process exhibits a contrasted trend; apparently it makes for synthesis and increasing diversification, for the production and increase of novelty and complexity. It may be argued that this is a general characteristic of the cosmic process; the world, in spite of thermodynamics, has achieved quite sufficient diversity and complexity, even if we disregard the distinctively biological developments. Nevertheless, the natural tendency to diversification—the "creativity," which Whitehead characterizes as ultimate[1]—appears to be greatly enhanced and to express itself most obviously and effectively in living organisms. Correspondingly we find in the latter a unique complexity of material organization, and in correlation with this the emergence of types of existence and activity which are absent or inappreciable in non-living nature. One of the tasks of a philosophy of science is to account for and reconcile the existence of these two contrasted tendencies in nature.

The facts of evolution, then, especially organic evolution, point to the existence of a tendency or *nisus* in nature making for diversity; *i.e.* some activity of a differentiating or individualizing kind is present in addition to the uniform or regularizing factors. A scientific difficulty enters here. In physical science events are

[1] *Process and Reality*, pp. 30 *seq.*

traditionally regarded as occurring in strict conformity with stable rules or uniformities; its view of nature has been based on modes of abstraction which disregard the possibility of departure from certain permanently established types of order. Undoubtedly the implied denial of spontaneous or arbitrary natural factors has had much to do with the more or less open rift long existing—now apparently in process of closing—between natural science and philosophy. To the philosophic mind creation has always seemed to imply teleology; and since a diversified world with living beings is a demonstrable fact, the problem of origins becomes a very real problem. We now see that there was no sufficient basis for this mutual suspicion. The rejection of teleology in pure physics is in fact to be regarded simply as a characteristic of method; it should be taken as asserting not the *absence* of teleological factors in nature but simply their irrelevance to the physical point of view. To the biologist, however, the facts of growth, diversification and evolution must inevitably be of very special significance in forming his general views of nature and the cosmic process. If we regard the diversifying element or agency in nature as identical with, or as having factors in common with, the trend or impulse which expresses itself in the evolution of living beings, we reach a biocentric view of nature, contrasted in many respects with the purely physical and allying itself perhaps most closely with that of Leibniz in the *Monadology*.

In a recent paper[2] I have classified the factors which determine the special features of any natural process or existent into the two broadly contrasted groups, *factors of stability* and *factors of change*. These correspond respectively to the conservative aspects of nature and the active, causative, or originative aspects. Scientific observation shows that in any natural existent the two are inseparable—like the opposite faces of a shield, or the warp and woof of a fabric. The conservative factors (factors of stability) are represented by the various natural constants; these define the static or invariant conditions within the system under consideration, *i.e.* the structural or constitutional permanencies which,

[2] American Naturalist, 1934 (in print).

although themselves unchanging, control the manner in which events happen. Such stable factors include the constant formal conditions (*e.g.* geometrical or purely mathematical conditions which apparently have a primordial or invarying stability), and the various physical constants; the latter appear in some cases as fixed or ultimate (like the constants of radiation, electricity or gravitation), and in other cases exhibit more special features, corresponding to characters possessed by the various stable products of evolution. This last group of constants (evolved constants) would include physical characters such as the constitution of the atoms; also various large-scale permanencies such as the distribution of astronomical bodies, the figure of the earth, the succession of geological strata, and stable biological characters like the ground-plan of the vertebrate body; and their number in the world as now constituted is extremely large. We recognize this when we observe our own dependence on the stable features of our own organisms and environment. The factors of change are also infinitely various in detail; but they always act under restrictions (defined in physics as limits, parameters, boundary conditions, etc.) imposed by the permanent or stable factors. It is because of the existence of the permanent factors that the various activities of nature fall into *types*; and the types of change appear to be at least as numerous as the constancies on which they are based. Complex types of change or activity require correspondingly complex permanencies as their foundation; the best illustration of this principle is seen in the structural, chemical and other features (*i.e.* the organization) subserving the activities of living animals. The very word type expresses permanency; it implies the existence of unchanging conditions or factors making possible the repeated production or recurrence of the object or event under consideration. Observation shows that this interdependence of constancy and change is a general feature of natural existence, apparently corresponding to some basic metaphysical condition. Without some underlying permanency of condition there could be no change; nothing could happen twice in the same way; no body would fall to the ground, no projectile would continue on its path, and equally (to give a biological

illustration) no living organism would develop in a constant manner from its germ. It is in the living organism that the continual presence of change in a background of stability appears most clearly as a general characteristic of natural existence.

A further general consideration is relevant at this point. While the natural constants determine the *character* (type) of the changes which are possible to any natural system, they do so only within certain limits; a certain range of variation is possible in every case. We recognize this when we say that no two physical objects are exactly alike. Complete likeness is an ideal, never an actuality, among natural existents; and similarly the determination of any single natural event or process by external conditions (*i.e.* external *to itself*) can never be complete. We refer here to the importance of internal (self-determining) factors in the behavior of any natural entity; later this feature of reality will be discussed in more detail; apparently it is to be referred ultimately to the monadic constitution of Nature.

The character of the change or activity possible in any special case is thus not fixed absolutely, but only within certain limits, by the various permanencies or stabilities which are proper to the system at the time of change. In other words, the factors of stability control or limit but do not completely determine the special course of the change. This statement defines broadly the general character of the conditions underlying the evolutionary advance of nature. We observe that this advance proceeds gradually and often by imperceptible degrees. The reason for this appears to be that each successive advance requires as the condition of its occurrence the previous attainment of some definite station which has a certain persistence or stability. Any such stable set of conditions, having once been reached, furnishes a foundation for further change; and this change may have the character of progress or novelty. In other words, acting *within* the stable conditions thus established in the past and controlled by them in the manner indicated are active factors, which are not completely determined but only *limited* in their operation by external or preceding (established) conditions; these factors manifest themselves in various novel types of change leading

ultimately to the diversification which we find in nature. The existence and characteristics of living organisms are to be regarded from this point of view.

When we apply these general principles to the problem of vital organization we are at once struck by certain features of a highly special kind which are peculiar to the living system. Structurally the latter is highly differentiated; this is evidently true of the visible or large-scale structure (or anatomy) of any animal or plant, but it is equally true of the minute or small-scale structure. It is important to realize this clearly, since the large-scale organization of any adult animal or plant is a temporary construction, built up by degrees through the activity of small protoplasmic units or "cells," each of which has a highly characteristic physical and chemical constitution. We infer, therefore, that the essential factors of vital organization are to be sought in the single cells. What is characteristic of even the simplest protoplasmic unit is its high degree of diversification—its lack of homogeneity, its minute differentiation within a small volume; in this respect it offers a striking contrast with systems which tend automatically toward uniformity of structure and composition, such as a droplet of solution. The elements of protoplasmic structure are *small-scale* in the physical sense; we assume, therefore, that small-scale factors play a special part in vital processes. Now in general what is peculiar to small-scale events is that with progressively finer and finer subdivision we ultimately reach a state where the actions of individual atoms become of greater and greater importance in the determination of the system.[3] When we attempt to specify exactly the behavior of individual atoms we meet with the restrictions defined by the indeterminacy principle. Certain actions which are determined by the individual or internal peculiarities of the atom cannot be defined or characterized in detail, although the mass effects when large numbers of atoms are concerned may be measured to almost any required degree of precision. The contrast between the mass effects of molecular activity (as summed up in molar mechanics and thermodynamics)

[3] *Cf.* the discussion by F. G. Donnan, *Scientia*, 1918, Vol. 24, p. 282; also Journal of General Physiology, 1927, Vol. 8, p. 685.

and the individual actions of single atoms or of small atomic groups acting in concert must always be borne in mind in considering the problem of vital action. According to the present view, the contrast between the activities of living and non-living natural systems, considered from the physical point of view, is it be referred to this difference in the scale of the primary determinative events, these events, in the case of the living system, being intra-atomic rather than molar.

The important further conclusion follows: vital actions differ from the actions of non-living systems—where typically mass factors preponderate and events are controlled mainly from outside—in that the special mode of action can be accounted for only to a limited degree, if at all, by reference to conditions external to the system under consideration. Determination is *internal* rather than external. This seems especially evident when we consider the most characteristic and unique feature of the living organism, its coming into existence as the result of progressive growth and transformation beginning with a minute germ and ending in the highly organized adult. When an egg cell is activated (by fertilization or by artificial means) the process of development is initiated by sub-microscopic changes occurring in the central region of the cell, the nucleus; it then proceeds automatically by a process of ever-increasing complication to the adult stage. The detailed character of the constructive process is controlled by intracellular metabolic reactions; these in turn are controlled by events occurring in special nuclear elements. We have here illustrated the traditional contrast between growth by intussusception and growth by accretion; the processes determining organic growth are primarily *chemical* processes, consisting essentially in an ordered series of reactions by which a chemically and physically complex system is built up. The ultimate causative reference is to changes occurring in single molecules or atoms, *i.e.*, to processes internally rather than externally determined. Can we define more closely the conditions determining this type of transformation?

Individual development or ontogeny is a process of synthesis of the complex from the simple, and to all appearances it is

epigenetic, *i.e.*, the full complexity of the adult is not present in the germ but appears by degrees as the outcome of an ordered process of increasing structural and physiological diversification. This constructive process follows a constant or stereotyped course and eventuates in a definite or constant organization, *i.e.*, one which is repeated with a high degree of exactitude in all normal individuals of the species. Constant guiding or stabilizing factors must therefore be assumed, which control the course of the transformation. Experiment shows that these factors are partly external (environmental) and partly internal (genetic). It is therefore possible to modify development in two ways, either by change of enviroment, or by change in genetic factors (*e.g.* hybridization or mutation). Mutations, the stable or heritable variations now regarded as the main source of evolutionary modification, may also be spontaneous or induced. It is significant that radiation of short wave length (high frequency or high quantum value) is apparently the most effective physical means of inducing mutations,[4] since this result points to an influence exerted on the atomic or molecular scale—atomic dimensions being of the same order as the wave-lengths of x-rays.

A general feature of protoplasmic organization is the control of the synthetic processes of growth and maintenance by factors having their *locus* in the cell-nuclei. The evidence of experimental genetics indicates that the factors determining the constancy of Mendelian inheritance, the genes, are situated in the chromosomes; physically they have the character of small bodies (*e.g.* 50 mμ) serially arranged in constant order; each of these can be shown to have a specific influence on the course of the developmental transformation, although in normal development the united influence of all is concerned.

As to the manner in which this influence is exercised we have no clear conception; the comparison with catalysis, while a favorite one, is on the whole misleading. On the basis of the general principles already discussed, we may say that the continued existence or reappearance of any definite (*i.e.* identifiable or

[4] *Cf.* H. J. Muller: "Radiation and Genetics," American Naturalist, 1930, Vol. 64, p. 220.

repeated) sequence or cycle in nature implies the persistence or stability of certain factors which determine its origination and its special character. Hence the constancy of the course of development in any species indicates the presence of controlling factors which remain unchanged from generation to generation. The reproducibility or stability of the ontogenetic process depends upon the constancy of these factors, which we therefore class as factors of stability. Through their presence the various chemical and physical changes which together constitute ontogeny are restricted to a definite course with only a narrow range of variation; the whole sequence of events has a stability or constancy corresponding to the constancy of the stabilizing factors. Of these factors the intranuclear genes appear to be the chief. The most appropriate single word to characterize this controlling influence would seem to be "directive;"[5] the constancy and progressive character of the diversification suggest the analogy of a system of fixed pathways or special directions along which the impulses to change are transmitted. In contrast to the tendency to symmetry or homogeneity which we find in non-living systems (such as a mixture of compounds in simple solution), the organic transformation is asymmetric; with this trend toward asymmetric action is connected the trend toward ever-increasing complexity. This asymmetry has its origin in atomic and molecular behavior, as is shown by the predominance of optically active or asymmetric compounds in synthetic metabolism, especially in the production of amino-acids, the "building stones" of proteins. In general we observe that in the synthesis of active compounds in protoplasm dextro- and laevo-molecules are formed in unequal numbers, or one is formed to the exclusion of the other. Pasteur, in fact, regarded this tendency to asymmetric synthesis as the chief distinction between the chemistry of living and of non-living systems. Apparently the structural complexity of protoplasmic organization, formed or built up as it is by the synthetic reactions of cellular metabolism, can only be

[5] *Cf.* R. S. Lillie: "The Directive Influence in Living Organisms," Journal of Philosophy, 1932, Vol. 29, p. 477; asymmetric chemical action and its relation to vital processes are more fully discussed in this paper.

understood as the outcome of a continual asymmetric or directive activity by which the physical and chemical influences and interactions are restricted to certain spatial directions. Some preexisting set of conditions must be assumed which counteracts the tendency to chemical and structural symmetry which is the typical result of forces distributed at random. In the living system randomness is replaced by some kind of directive influence, originating intra-atomically and acting on the atomic or molecular scale.[5]

We can form no physical conception of these directive influences on the basis of the classical physics, which regarded atoms as inert bodies acted upon passively by purely external forces. In order to account for the complexity and apparently directive character of the inter-atomic actions in the living system, we seem obliged to take into consideration the known complexity of intra-atomic organization. The scheme of classical physics has a simplicity which, although in many respects sufficient for the characterization of large-scale phenomena (a good example is the kinetic theory of gases), has proved quite unadapted to accounting for the behavior of matter on the atomic scale. *A fortiori* it is unadapted to elucidate the intimate processes of living protoplasm. The development of quantum mechanics is at present furnishing the needed theoretical foundation for atomic physics; and its application in biology, now only beginning, will undoubtedly lead to results of far-reaching importance.

What additional factors must be assumed to account for the directive activities of the living organism? The physiologist will think of possible physical factors not yet discovered, *e.g.*, intra-atomic factors controlling the time and direction of quantum transfer; the psychologist may point to psychical factors, after the analogy of directive voluntary action in human beings; again the vexed question of the psychophysical relationship will arise. Here it is not possible to consider this question, beyond noting that in the living organism (any human being) observation shows the existence of both factors. To reduce the one kind to the other seems impossible. Here, if anywhere in natural science,

Bohr's principle of complementarity has its application.[6] It seems clear that the sharp separation of physical from psychical in ordinary conception cannot correspond to a primary division or dualism within nature itself, but is a result of abstraction, fixed by verbal usage; custom emphasizes the contrast and disregards a possible unity. But (as Prinzhorn has recently remarked) the concepts "purely psychical" and "purely physical" must be regarded as fictions;[7] if we accept this conclusion, any real natural existent must be regarded as a compound of—or as including within itself—factors having characters of both kinds. The single atom would be such an entity; intra-atomic directive factors could not be designated as either exclusively physical or exclusively psychical. A unification, for which we have no single term (unless psycho-physical), would have to be postulated. Here again the Leibnizian monad with its insulated and discontinuous character and internal activation is perhaps the most adequate general conception on which to base a theoretical reconstruction, for it recognizes the atomic or isolated character of the ultimate natural units and regards the source of their activity as primarily internal and as having a psychical as well as a physical side. Leibniz's theory is in fact definitely biocentric.[8]

Let us return now briefly to some of the more special yet scientifically definable and stable features in the physical constitution of the living system and try to form some conception of their significance in the total vital activity. I have already referred to the fine-grained structural conditions in living protoplasm, to the minute size of the genes, and to the predominence of asymmetric reactions in synthetic metabolism, especially of proteins. These are features which point to the importance of intra-atomic types of action; such actions have a certain asymmetric or directive character as distinguished from mass actions in which the random motions of many individual atoms annul or compensate one another. The spontaneous development of a high degree of small-scale structural differentiation—the unique

[6] *Cf.* N. Bohr, "Life and Light," Nature, 1933, Vol. 131, pp. 421, 457.
[7] H. Prinzhorn, "Psychotherapy," London, J. Cape, 1932, p. 97.
[8] *Cf. Monadology*, 66 *seq.*

characteristic of protoplasmic processes as contrasted with those of non-living chemical systems—seems explicable only by reference to intra-atomic factors of determination.

There is a further highly characteristic feature of the living, especially the higher, organisms which now requires consideration. Although the special directive factors of small-scale vital activity are localized intra-atomically (according to our conception), the vital activity does, in fact, express itself ultimately on a large scale, as shown in the building up of the adult organism with its complex but unified structure and behavior. Conditions of a stable or regularly acting kind must exist to provide for this unification. We come now to the all-important biological problem of integration. This problem has its reference to the single protoplasmic unit or cell as well as to the highly organized adult individual. Let us consider first the nature of the unifying factors in the single cell. A detailed physiological discussion is not possible here, but the nature of certain general conditions may be indicated briefly. First, we may point out the fundamental importance of electrical conditions; these appear to supply the chief factors providing for the unification or synchronization of spatially separated processes within the cell or in adjacent cells. Variations of electrical potential occur at the numerous interfacial boundaries and surface-films which are such important features of protoplasmic structure, and these give rise to local electric currents which flow in paths determined by the special structural and other conditions in the cell-system. Such currents will have chemical effects through their influence on the electron-transfers between molecules at the electrode-like boundary surfaces (the "chemical distance action" of Ostwald); they also form the basis of transmission of the nervous type, the chief means of integration in higher animals. Another well-recognized general condition, also dependent on the polyphasic structure of protoplasm, is the tendency of polar molecules to become oriented in definite positions at boundary surfaces, thus forming layers of molecules with their similar regions adjacent to one another. Many structural and physiological integrations have almost certainly their foundation in this tendency, which is also an

important factor in the electrical sensitivity and the catalytic activity of the system. In any system consisting of large and complex molecules disposed regularly in a microscopic structural framework of constant pattern, the possibilities of complex action are at their maximum. It must also be remembered that each chemical component of the protoplasmic system, *e.g.* a protein or a lipoid body, has a certain static complexity (of structural, chemical and other character) proper to its special composition; and this static complexity determines the complexity of action which is possible to it as a participant in the general activities of the system. It is thus no accident that the purely chemical (*i.e.* molecular or small-scale) composition of living organisms is complex. Such complexity is a necessary condition for complex activity; in general, as already pointed out, complexity of static condition is an essential prerequisite for complexity of ordered or regular action. Any organization of a highly complex type can maintain or support correspondingly complex activities only so long as its structural foundation preserves the necessary constancy or stability. This is a general principle seen in all manifestations of life, from the simplest cellular processes to the conduct of nations. Hence in all living organisms the *maintenance* of the complex structural and other organization is of equal importance with its production in development. Since the cell is a metabolizing system, the protoplasmic materials and structure are subject to continual breakdown, which is offset by continual automatic reconstruction and repair. Organic structure is thus actively maintained structure, as Haldane and others have pointed out; maintenance requires ceaseless physical and chemical activity, adjusted at the required level by automatic regulations of various types. To discuss these in detail is not possible in this paper.

In general, when we use the term structure or organization in reference to any complex natural system or process, we mean the permanent existence of a definite set of relations, constant or reproducible, between its component parts or activities; we also assume that these components are themselves constant in their characteristics. Thus the term has reference to factors of stability and their combination. Structure or organization, in this

sense, is assumed to be at the basis of any integrated complex activity. And since integration is the most characteristic feature of vital activity, the problem of vital organization becomes largely equivalent to the problem of integration. This has its strictly objective or physiological side (morphological, nervous, hormonal and other factors of integration); on the psychological side the vital integration exhibits itself in the unity of bodily feeling, in the various forms of "Gestalt" phenomena, and finally in personality. In its most general aspects the problem of integration relates itself to the problem of teleology and through this to other problems of general philosophy.

Integration in ontogenetic development has been the subject of extensive physiological and experimental study. The constancy or reproducibility of the developmental sequence is evidence that its initial stage has a definite and stable structural foundation; this is regarded as consisting in certain constant features of the germinal organization, as already indicated. Development usually begins with a single cell having a constant physico-chemical and morphological constitution; this germ grows by the accumulation of materials taken from the environment, and the whole system as it grows undergoes progressive diversification of a specific kind. In the developing embryo each stage as it is reached furnishes the foundation of the next stage; each stage has therefore a certain persistence and stability which, however, may be only relative, *i.e.* it may last only long enough to lay the foundation of the next stage.[9] Exactly what occurs at any stage is a function of the chemical composition of the system at that stage, as well as of its morphological structure; the two, in fact, are inseparable, since organic structure is a product of the synthesis of definite chemical compounds which are deposited in definite situations and structural interconnections. The morphological and the biochemical sequences of development thus go necessarily hand in hand.[10] Certain constant physiological and biochemical interdependences between successive processes are

[9] Provisional or "scaffolding" structures are frequent in embryonic development, *e.g.* the amnion and allantois of the mammalian embryo, yolk-sacs, etc.

[10] For a detailed account of the chemical side of embryonic development *cf.* the recent treatise of Joseph Needham, "Chemical Embryology," 3 volumes, Macmillan, 1931.

also well established experimentally; among these are the dominance and organizer influences (investigated in detail by Child and Spemann) and the various types of hormone influence. For normal development a certain constancy of environmental conditions is also required, and (in later stages at least) the maintenance of normal physiological activity. Analogous considerations apply to adult life, since no hard and fast distinction can be made between regulation in embryonic development and the preservation of physiological balance in the adult. In both cases a constant interdependence of highly diverse processes is provided for and a variety of means is employed to secure this end. Automatic adjustment or *regulation* is a typical feature of vital organization at all stages, and its physiological factors are becoming better known as research advances.

Yet the intimate nature of the developmental and organismic unity remains obscure, and ultimate explanations tend to become metaphysical. This seems unavoidable; all possibilities must be considered, and even when scientific criteria are applied most rigorously, philosophical curiosity tends to outrun the results of analysis. We must recognize that the problem of the unity of the living organism, as manifested either in its ontogenetic development or in its adult life, leads back inevitably to the general philosophical problem of individuality.

Again we come to the monadism which is, at least in the form given to it by Leibniz, an essentially biocentric conception. The impulse eventuating in life must be deep-seated in Nature, and it seems necessary to recognize its existence even in the atoms—at least in the sense indicated above, that the vital directive factors are ultimately intratomic. We must avoid being mystified by the term "life," and no one would assert that an atom is "living" in the same sense as a bacterium. Nevertheless, the tendency to diversification, synthesis and individualization appears to be a fundamental fact in nature; and this tendency cannot be accounted for solely by reference to uniform conditions, or to the values of the fundamental natural constants. Some term should be agreed upon to designate this tendency. Whitehead has used the word *creativity* to describe the diversifying and novelty-producing trend in nature; creativity is an essentially active character—one, how-

ever, which works under restrictions of a more or less permanent kind; these are partly primordial (to use Whitehead's phrase), and partly imposed by those stable factors or conditions which have established themselves from time to time during evolution (derivative or consequent). Living beings, with all their multifarious characters, which we must accept and cannot explain away by analysis, are an expression or outcome of this creativity.

The problem of vital organization thus leads ultimately to philosophical considerations of the most general kind. In the present discussion we have attached chief importance to directive factors whose special nature is determined by the internal constitution (organization) of the living organism or its germ; and since the properties native to the organism cannot be regarded as stopping short at the boundaries of the atoms, we have regarded this internal constitution as including that of the atoms. Evidently the atoms composing the organism do not act independently, but in a concerted or unified manner. Logically we should infer that the inner constitution and activity of each of the many atoms associated in the complex vital organization would be affected by the fact of that association.[11] Such an inference may be beyond the possibility of experimental testing at present, and by some criteria would be regarded not as scientific but as metaphysical. Reference to internal factors is, however, unavoidable if we are to account for the special properties and behavior of any individual being, living or non-living, since the internal constitution of any real thing is what determines its constant modes of action under any given circumstances. This general consideration applies to the internal constitution of an atom as well as to that of a living organism.

The question arises how this or any other kind of internal constitution is to be regarded as originating and how it is most adequately to be conceived. Does the conception of internal constitution imply necessarily a spatial arrangement of parts? If so, the implication would be that all reality has a spatial character. The atom would be conceived as consisting of space-

[11] *Cf.* Whitehead, "Science and the Modern World," p. 111, as to the influence which the general unity of the organism exercises upon the behavior of the single electrons composing it.

occupying entities connected in spatial inter-relationships and exhibiting activity in time. But there are difficulties in this view, since it involves logically an indefinite regress when we attempt to spatialize each of the component entities in its turn. Or is spatiality an essentially formal rather than a real character, as Leibniz and Kant maintained? The present spatial models of the atom must be recognized as artificial constructs, more or less arbitrary; and in fact other modes of formal representation are now being substituted in atomic physics as being more in accordance with experimental fact. This is not the place for a discussion of the metaphysical question. We might point out, however, that the monad of Leibniz is not to be regarded as an object occupying space, but rather as an isolated center of existence and activity (or tension) to which spatial modes of conception apply only within certain limits. Thus when we attempt any representation of the monads in their simultaneous relationships, a spatial construction is required; but this type of formalization need have little application to the inner reality (or selfhood) of the monads themselves. The biological conceptions of vital organization, which are necessarily based on the accepted methods of scientific procedure, are of course spatial, and as such are subject to the continual test of physical experiment. Undoubtedly future biological research will define with an ever-increasing fulness and precision the constant physical conditions of vital activity; nevertheless, we must recognize that this activity, considered in its entire character, is not completely limited or determined by the physical or other constant conditions under which it manifests itself. It includes also the element of free activity or creativity, with its constant feature of novelty or independence of external and past conditions. Hence we may predict that evolution will continue to give rise to new forms of life; and in the human sphere we must acknowledge the very real sense in which individual men and women are free agents, within the restrictions set by their biological organization and conditions of life.

Department of Physiology,
University of Chicago.

The Views of Haeckel in the Light of Genetics[1]

BY

H. J. MULLER

THE extent to which Ernst Haeckel was hated and attacked by reactionary scientists, philosophers, litterateurs and preachers, is, as Lenin has pointed out in his "Materialism and Empiro-criticism," a measure of the success with which he originally expounded the results of natural science and drove them home to their logical conclusions in the interpretation of nature on a materialistic basis. Haeckel, in the days of his greatest mental vigor, made himself the spear-head of the scientific attack upon the then dominant forces of theology and obscurantism, showing fearlessly how revolutionary were the results of modern science, and particularly those concerned with evolution, in altering fundamentally man's outlook upon the world in which he lived and his own nature. It has become the mode among many intellectual circles in Western countries to refer slurringly both to the more technical scientific work and to the more general reasoning of Haeckel, as being crude and out of date; but this derogation has its origin in the despair of the teleologists and is not founded in the facts of modern scientific development. Certainly, so far as genetics is concerned, the most modern results are not merely in harmony with, but brilliantly confirmatory of, those main tenets of Haeckel's general interpretation of nature which involved him in the bitterest con-

[1] Based on address given at Haeckel Centenary of the Academy of Sciences of the U. S. S. R., in Leningrad, Feb. 16, 1934.

troversy. In addition they help to show the general soundness of his more technical zoological contributions. By contrast, Haeckel's deficiencies in the realm of social affairs stand out the more strongly, although his own materialism of natural science is a necessary basis for an adequate social materialism. We will not concern ourselves here, however, with Haeckel's own social theories, but only with those in the field of natural science; it was only in the latter field that he attained his real prominence.

Turning first to the more technical work of Haeckel, we may note that his pioneer achievement in tracing out a single evolutionary (phylogenetic) tree wherein all higher organisms are interrelated, has to a surprisingly large extent stood the test of time, and the methods which he used in constructing it thus stand vindicated. His was the first great classification of organisms which was made upon an essentially sound basis. In every such great work there must be some slips, and the wonder is (in this as well as in Haeckel's more general scientific work) that there were not many more. Certainly when we consider his work in phylogeny in comparison with some of the more recent flounderings of lesser biologists, as, for example, the recent attempt of some paleontologists (in a misdirected endeavor to maintain their own respectability) to derive man from something other than an ape, or the related and still wilder hypothesis of a separate origin of each main subdivision (phylum) of animals, our regard for the balanced judgement of Haeckel increases. The findings of genetics concerning the identity of the genetic mechanism in all higher organisms, as well as facts of immunology and of chemistry (e.g., the like optical rotation of the glucose of all organisms—an item to which J. B. S. Haldane has called attention) confirm Haeckel's portrayal of living things as all derived from one original stem. And the present conceptions of genetics concerning the nature of the evolutionary process (which involve a considerable element of accident and of unique determination in the occurrence and establishment of mutations), indicate that in general the morphological characters of the taxonomists are valuable evolutionary guides, in that similarity in whole combinations of relatively unconnected characters of this sort would not

come about by their independent origin and convergence in distantly related groups, but must point, as Haeckel assumed, to the existence of common ancestors possessing these combinations of characters. This principle automatically leads to the conclusion that all races of man arose from one *human* stem, that the remote ancestors of men must have been monkeys, mammals—reptiles, etc.[2] While it is often true that the minutiae of the connections between numerous closely related forms can not be worked out by these morphological (including embryological) methods, nevertheless the broad outlines of the evolutionary movement can be traced in this way and also many details, and it is very useful as well as enlightening to have this information.

As concerns Haeckel's contributions to embryology in its relation to evolution, it has become popular in some quarters to ridicule his "biogenetic law," according to which the development of the individual involves a repetition of the successive stages which have previously been passed through in the evolution of the organism. The geneticist would of course admit that this method of statement was rather a *dramatization* of the embryonic events than an exact description, but it would be much further from the truth to cast aside Haeckel's principle entirely than to accept it as it stands. Certainly it is obvious that, for example, there could have been no cocoon-like stage in the past evolution of the butterfly, and therefore not only adult but also embryological stages may be subject to a great amount of modification in the course of evolution; but it is certainly true also that in most organisms the embryo shows far greater resemblances to ancestral

[2] Thus, to illustrate the second statement, the striking agreement between man and the higher apes in numerous characteristics which distinguish them both from ordinary monkeys shows that the common ancestor of both was less remote than an ordinary monkey; but, on the other hand, the agreement of the higher apes with the ordinary monkeys in many non-human particulars which, taken together, would label their possessors as, in a more general sense, monkeys, shows that the above common ancestor of apes and men must itself have been, in a more general sense, a monkey, of a tree-dwelling sort (though belonging, more specifically, to the ape subdivision). In a similar way, it can be shown that this ancestral ape must have had a common ancestor with the "lower" old-world monkeys, less remote than the new-world monkeys, and that this ancestor, if living today, would be classed in the group of ordinary old-world monkeys (since the more specifically un-ape-like characters are common both to old-world and new-world monkeys).

forms than does the adult, and that there is a tendency to a similarity in the chronological sequence, the earlier stages in the embryo having a great similarity to the earlier ancestral forms. This phenomenon is not merely a result of the fact that both individual development and evolution must proceed from the single cell to the many, and in general from the simpler to the more complex, but it has also a more special basis, of which we have only recently begun to demonstrate the genetic cause (though August Weismann, characteristically enough, much earlier divined the essentials of the explanation). According to genetic theory, evolution proceeds through the survival of mutations that happen by accident to be beneficial; and since mutations having large effects must have more chance to be disadvantageous (as we find in fact to be the case), it is easily seen that they would less often survive. Now if an inherited mutation produces an effect early in the development of the individual, this effect will be likely to have other effects in turn, in the succession of developmental stages, and so to have produced larger or more diversified changes by the time the adult condition is reached. On the other hand, mutations of such a nature that the change caused by them begins relatively late in development, will be more likely to be circumscribed in their final effect, and hence less likely to be lethal or harmful, and more of these will therefore become established in evolution. An analogy could be drawn here to the process of remodelling a house: alterations in the foundations or other primary structures, which were laid down earlier and about which the rest has been built, are difficult to accomplish without much destruction, whereas parts can relatively easily be appended successively to the already completed structure. Thus on the whole there will be a tendency for the earlier stages of the development of organisms to remain more nearly of the ancestral type, while the changes that became established later and later in the course of evolution will tend to manifest themselves as though added on to the later and later stages of the individual's development. Of course many exceptions to this general tendency can and will occur, but it will nevertheless hold true as a tendency, and it will thus justify,

although in a less sweeping form, the generalization of Haeckel's which he rightly found so helpful in constructing his evolutionary tree.

Unfortunately Haeckel did not supplement his anatomical and embryological studies of evolution by experiments and observations on heredity and variation, and so he did not directly further the advancement of genetics, but he lived at a time when the significance of detailed studies of this kind for evolution were hardly realized by biologists. If Haeckel had carried his own logic far enough he would have appreciated the importance of such work, but I have been informed by one of his now most distinguished students, Dr. Oscar Vogt, that when the latter expressed his interest in carrying on such experiments, Haeckel told him that there was really nothing left to be studied in this field! Nevertheless, Haeckel himself was the first to make the suggestion, as early as 1866, that the nucleus was the carrier of the hereditary material, a fact which has since been established as a cardinal principle of genetics. He seems to have been led to this conclusion through a sound inductive process, upon the basis of the fact that the sperm, like the egg, carries a nucleus but little else that could conceivably subserve the function in question.

As the most vigorous of the early opponents of vitalism, Haeckel recognized that the basic problems of living matter are at bottom chemical, and that living things have been derived from non-living through the operation of natural chemical processes that resulted first in a very primitive protoplasm, which later evolved into cells ("Monoren"). Genetics treats the same problem nowadays somewhat more specifically, by pointing out the importance in the origin of life of the origination of genes, but Haeckel's more general formulation is inclusive of our viewpoint. If pressed, Haeckel would no doubt have agreed with modern geneticists that the detailed solution of the matter awaited a more refined biological chemistry, for he was assiduous in calling attention to the application of physico-chemical laws in the organic world, and to the triumphs of the chemist and physicist in analysing even organic material. Of course, since Haeckel borrowed his notions of physical science from those prevalent in his day, he

had far too limited and "mechanical" a view of molecules, atoms and physical realities in general; the important part of his work, however, was not his interpretation of physical phenomena as such, but of biological phenomena in terms of physical ones. And though he could not realize, as we can today, the profound complexity of the physico-chemical processes of protoplasm, nevertheless he mustered the evidence, in a masterly and essentially correct fashion, to show that the phenomena of living things did not call for an explanation in terms of any forces or essences that were not resultants of physico-chemical forces and materials. The further biology has advanced, since Haeckel's day, the more evidence has accumulated for this conclusion, as against the mysticism of the vitalists.

The most important general biological work of Haeckel was, first, his espousal of the theory of organic evolution—a battle universally conceded to have been won—and, second, his championing of Darwin's explanation of the process of evolution through the natural selection of accidental variations. Guerilla warfare is still being carried on in connection with this second battle by some non-biologists and by biologists in fields farthest away from the study of the processes actually at work in species change; but in the realm of genetics, which considers itself as lying at the focus of modern evolutionary inquiry, this second battle also has been won. Darwin's explanation found its chief difficulty in the supposed gradual obliteration of new variations through their crossing with the original type; but modern genetics has demonstrated this obliteration to be illusory, since the individual units, the genes, are inherited uncontaminated and unchanged—except for their rare sudden mutations, which provide the material of variation required by Darwin and Haeckel. The stable inheritance of these variations being thus demonstrated, the natural selection mechanism must automatically result in the fitter ones being used in the adaptive evolution of a population.

Of prime importance in this natural selection theory was the principle that the inheritable variations were not *purposive adaptations*, but that their adaptiveness was only accidental,

when it existed at all, although it was of course through the survival of those germ plasms which did have adaptive variations, and the dying out of the others, that evolutionary adaptation advanced. This principle is absolutely essential to a naturalistic, non-idealistic explanation, and a demonstration of it cuts away the ground from under all teleology in the origination of the organic world. It is therefore the crux of the whole controversy and the kernel which the theologically or idealistically minded have found it hardest to swallow, so that ever since Darwin they have tried to evade it and to smuggle in some alternative scheme of hereditary variation; it can be shown that all these alternative schemes, such as orthogenesis (including Osborn's "aristogenesis"), inheritance of acquired characters, direct action of the environment, etc., must ultimately rest upon the idea of a guiding purpose. It was precisely the unpurposefulness of the process by which evolution occurred, which Haeckel, even more than Darwin, was so zealous in insisting upon, so far as the knowledge of the day would allow him to. But it is only to-day, relatively speaking, that we have been able, through the findings of genetics, to demonstrate empirically and exactly the correctness of this non-teleological, materialistic conception of variation and evolution. For this it has been necessary first to show a point of which even Haeckel and Darwin did not properly realize the significance: namely, that the directly adaptive changes which do admittedly occur as a result of the physiological and morphogenetic reactions of the body (soma) of organisms, are not inherited, they themselves being only the product, and not the source of evolution. If this adaptive ability had been pre-evolutionary, then life would indeed have had to involve some purpose, "entelechy," or "perfecting principle" at its foundation; but genetics, as a result of the most assiduous studies, has found no trace of this fabled "inheritance of acquired characters." Secondly, genetics has been able to show positively that those variations which are inheritable, that is, the mutations of the genes, are, in the large majority, not merely not adaptive, but positively harmful to the organism, in fact deadly to it (lethal). This is just what we should theoretically expect to be true of

changes *accidentally* occurring in the basis of so complicated an organization as a plant or animal. It should be, and is, only by a very rare accident indeed, that a mutation results which may be useful to the organism. Moreover, on account of their known source of instigation, those mutations which must be universally acknowledged to occur as a result of non-adaptive, "accidental" disturbances, namely, those produced by Roentgen irradiation, are, on the average, no more harmful and no more frequently harmful than the natural mutations of unknown origin, which provide the ore from out of which the valuable materials of evolution are sifted. The conclusion follows that the natural mutations are equally accidental in character.[3] No more room is left, then, for the existence of one bit of purpose in the origination of those natural mutations that form the building blocks of evolution. This finding is the last stone that was still needed to fill out the demonstration of the correctness of that general conception of the evolutionary process for which Haeckel struggled. Far from genetics having overthrown the essential principle of Darwinism, or even cast doubt upon it, we see that it has substantiated it, and, in so doing, it has greatly strengthened all the materialistic conclusions which Haeckel derived therefrom.

Haeckel did not rest content, as Darwin apparently did, with merely drawing his biological conclusions, but he militantly employed these conclusions in an attack upon many current conceptions with which they were irreconcilable, and this of course primarily meant attacking the ideas of animism and theology in their various expressions. It is true that Haeckel himself unfortunately did not call his doctrines materialism, nor did he claim that they were opposed to religion as such; in fact he allowed his at first more or less meaningless but potentially dangerous formula, "Monism," eventually to drift into an unmistakably theological form. Nevertheless he did, in his days of lucidity, attack the existing theological conceptions very clearly and vigorously, and showed their irreconcilability with the results of biology, and any consistent reader was straightway driven to materialism. As Haeckel pointed out, the establishment of the

[3] H. J. Muller. "The Method of Evolution," Scientific Monthly, 29: 481–505; 1929.

Copernican and Newtonian theories and the other achievements of science had, by the middle of the nineteenth century, left only two important series of problems that still seemed to require an answer in terms of purpose, firstly, those concerning the origin and functioning of the mind, and secondly, those concerning the method of origination and development of organisms in general. In the first field, however, the anatomists and physiologists soon adduced evidence for a fundamentally materialistic interpretation of mind as a function of the neurones of the brain, and the second field thus remained the chief stumbling block, since organisms were obviously adapted in the most pointed as well as elaborate fashion. Kant had regarded these facts of organic adaptation as irrefutable evidence of a purposive creative process. The clearing up of the seemingly insoluble problem of how all this purposefulness could have come about without the operation of purpose in it causation, by means of the relatively simple process of accidental variation and natural selection, thus demolished the last great fortress of the anti-materialists.

Since the issue of materialism and theology is so closely connected with the social struggle, by reason of the function of religion in the preservation of a class society, Haeckel's work was chiefly championed by the protagonists of the working class and attacked by the opposite side. In this way, as Lenin has said, the social significance of Haeckel's work was made clear, despite Haeckel's own attitude. Although Haeckel himself, as a purely natural scientist, failed in developing his conclusions to this stage, and, as a matter of fact, applied them absolutely improperly in the social field, where he finally showed himself to be an extreme reactionary (we may recall, for example, that he was one of the signatories to the infamous document of German professors endorsing Germany's side in the war), nevertheless his work helped to furnish the broad and sound background of materialism in natural science indispensable for a well rounded materialism of social science. And the campaign against his contributions may be regarded as a part of the general movement for the dragging back of all the discredited gods of the past century onto their battered pedestals, to serve as backing for the whole indi-

vidualistic and "bourgeois" conception and practice of life. But although Haeckel's contributions are still being attacked, they form in reality a well-established base that already lies well in the rear of the lines of the most pressing intellectual struggles of today. If we would follow the best in Haeckel's example, we should spend most of our energies in the work of positive advance, utilizing the principle that, where aggression is successful, defense sinks to secondary importance.

Institute of Genetics,
Academy of Science,
Leningrad, U. S. S. R.

Freud and the Scientific Method

BY

J. F. BROWN

ALL of Freud's more recent writings[1] extol the virtues of the scientific method. "The Future of an Illusion" ends with the definite suggestion that religious instruction be taken from the list of topics taught every child and that in its place children be taught to interpret reality in terms of natural science. A similar theme runs through "Civilization and its Discontents"; if there be any hope for humanity, it lies in the scientific method. Finally, Freud's "New Introductory Lectures" ends with the following lines—lines which must be very reassuring to many admirers of Freud (and the present writer counts himself one of these) who have been somewhat discomfited by some of the recent highly speculative passages from his pen:

Psycho-analysis is not, in my opinion, in a position to create a *Weltanschauung* of its own. It has no need to do so, for it is a branch of science, and can subscribe to the scientific *Weltanschauung*. The latter, however, hardly merits such a high-sounding name, for it does not take everything into its scope, it is incomplete, and it makes no claim to being comprehensive or to constituting a system. Scientific thought is still in its infancy; there are very many of the great problems with which it has as yet been unable to cope. A *Weltanschauung* based upon science has, apart from the emphasis it lays upon the real world, essentially negative characteristics, such as that it limits itself to truth, and rejects illusions. Those of our fellowmen who are dissatisfied with this state

[1] S. Freud. The Future of an Illusion, New York, 1928. Civilization and its Discontents, New York, 1930. Neue Folge der Vorlesungen zur Einführung in die Psychoanalyse, Vienna, 1933. The "Neue Folge" is also now available in English.

of things and who desire something more for their momentary peace of mind may look for it where they can find it. We shall not blame them for doing so; but we cannot help them and cannot change our own way of thinking on their account. (Sigmund Freud, New Introductory Lectures on Psychoanalysis, New York, 1933, pp. 248–9.)

The purpose of this paper shall be to inquire to what extent Freud's Psychoanalysis may be reckoned "a branch of science", to analyze certain Freudian concepts and methods, and to evaluate some of Freud's postulates and conclusions in the light of recent advances in scientific methodology. Before I proceed to this analysis let me make clear certain limitations of my criticism. I in no way plan to question Freud's factual findings. Nor do I consider myself competent to pick out flaws in the technique of the psychoanalysis of individuals which furnishes the Freudians with their data. Nor will I be concerned with the "truth" of the relationships alleged to exist between basic urges, their frustration, and the symptomatology of mental disease. These things may well be just as Freud says they are.[2] Relationships, which on discovery seemed equally bizarre, are now established in other sciences. I shall be concerned rather with questions like the following: Is the *type of theory* into which Freud has forced his monumental discoveries the best type of theory into which they could be forced? How is Freudian theory related to the type of theory used by modern physics? Could our knowledge of psychodynamics be furthered by a different type of theoretical approach?

To answer these questions we must see what we mean by different types of theory. It is quite obvious that physics is the best established of all our scientific disciplines. Physics is able to predict and control natural events. Biology and psychology are able to do this only to a very limited extent. This fact alone gives physics a very enviable position in the hierarchy of the sciences. Quite often psychologists and biologists have at-

[2] Early criticisms of Freud from moral and ethical standpoints are, of course, not worth considering at all. Today even some scientific criticisms of Freud like Knight Dunlap's (Freudism, Mysticism and Scientific Psychology, St. Louis, 1920) or even W. McDougall's (Outlines of Abnormal Psychology, New York, 1926) more carefully considered criticism seem somewhat ridiculous. My own very limited clinical experience has convinced me that the Freudian mechanisms exist and operate under *existing* environmental conditions in very much the way Freud claims they do.

tempted to follow physical methodology without realizing just what physical methodology is. Recent researches in the philosophy of science have done much to clarify such problems. In fact certain recent methodologists have gone so far as to suggest that the physical method and the physical language are (or, at least, should be) the method and language of science.[3] Our immediate task will be to examine physical method and theory in the hope of making certain generalizations about this methodology. Then we shall be able to see how far the Freudian Psychoanalysis meets the requirements for "good" theory. In doing this we shall use the method of "vergleichende Wissenschaftslehre" (comparative methodology of the sciences) which has recently been used with great success by K. Lewin.[4] We shall compare "class theory" which may be looked on as the antithesis of "good" methodology with "field theory." We shall set up ten criteria for both "class" and "field" theory. A word of warning is necessary in order that the designations "class" and "field" be properly understood. We do not mean to imply that our criteria will be solely concerned with the logical, physical, or mathematical properties of fields or classes. The central idea of a "field" theory is that the behavior of an object within the

[3] *Cf.* K. Lewin, The Conflict Between Aristotelian and Galileian Modes of Thought in Contemporary Physics and Biology. Jl. of Gen. Psych., 5, 141–177, 1931, and R. Carnap, "Die physikalische Sprache als Universalsprache der Wissenschaft," Erkenntnis, 2, 432–465, 1931. Also O. Neurath, "Soziologie im Physikalismus," the same 393–431, and H. Feigl, "The Mind-Body Problem" to be published in "Philosophy of Science." Although Lewin and Carnap and his followers disagree on certain points they are working in the same direction. It would take us far beyond the limits set for this paper to go into these differences here.

[4] For the idea of "vergleichende Wissenschaftslehre" see K. Lewin, Ideen and Aufgabe einer vergleichenden Wissenschaftslehre. By "class" theory I mean essentially what Lewin has called Aristotelian theory, and by "field" theory what Lewin has called Galileian theory. Similarly Bertalanffy (Theoretische Biologie, I, p. 26, Berlin, 1932) speaks of "vorkopernikanische" and "nachkopernikanische" periods in the development of sciences. I prefer the designations "class" and "field" for the following reasons. The biological sciences, particularly psychology and sociology, make use even today of concepts which are primarily "class" concepts and all of the implications of "field" theory became clear only very recently. Although the "field" idea is implicit in Galileo and Newton it does not become explicit until the time of Clerk Maxwell. I find my students for this reason become somewhat confused by the designation "Aristotelian" and "Galileian." Despite this change in nomenclature my very heavy debt to Prof. Lewin will be obvious to those of my readers, who have also read his theoretical papers.

field is determined by the field structure or spatial-temporal configuration of the energy within the field. The central idea of a "class" theory is that membership in a certain class determines the behavior of an object. Closely connected with the ideas of class and field one usually finds certain ideas on what constitutes the scientific method, the relationship of theory to fact, etc. It is necessary to point out that *not all* modern physics is field physics. The whole trend of modern science, however, is from a method which fulfils all the criteria of "class" theory to a method which fulfils all those of "field" theory. The designations "class" and "field" are used to indicate types of thinking rather than in their technical sense as used in logic, physics, and mathematics. Furthermore, these criteria are not to be looked on as logically or methodologically final or of equal value. Certain of them are implicit in the idea of "class" and the idea of "field" and may be derived from these ideas. Certain are concerned with the theoretical framework of systematized science, others are concerned with actual "fact findings." It is relatively easy to reduce the number of criteria to considerably less than ten or to spin it out to considerably more. The list as given, however, has proven itself valuable to me in examining various theories of psychology and biology. It is to be looked on as *tentative* and of chiefly *heuristic* value. In the following exposition I shall in two columns contrast "field" theory and "class" theory. After each criterion, we shall see how far Freud fulfils it. A certain knowledge of Freudian theory is, of necessity, presupposed. I have space for only a few examples from Freud's writings. Readers familiar with the Freudian system in all its ramifications will readily think of many more.

Criteria for Class Theory	*Criteria for Field Theory*
1. *The behavior of objects is determined by the "class" to which they belong.*	1. *The behavior of objects is determined by the structure of the "field" of which they are a part.*

In a "class" theory, the characteristics of behavior common to different individuals of a class are abstracted. If the behavior of

another individual shows these characteristics, it is included in the class, and the behavior is then regarded as explained. In explaining an event in a field theory, the structure of the field is characterized in terms of laws, which we shall see are logical constructs. If the behavior of the object follows these laws, it is said to be explained. Following Lewin we shall elucidate our criteria with examples from Aristotle's kinetics and the modern physical treatment of the same topic. Aristotle's kinetics depend on his chemistry. Earth, water, air, fire, and ether have their "rightful" positions in this order from the center of the cosmos outward. Whether an object moves upward or downward depends on the direction from which it has been removed from its "rightful" place. If fire is generated on the surface of the earth it represents a displacement and it moves upward because it belongs to the "class" fire. A stone thrown into the air, belonging as it does to the "class" earth, returns downward.

According to the modern physicist bodies fall in a direction and at a rate which is determined in final analysis by the spatial-temporal distribution of bodies in the cosmos (or, if we insist on an up to the minute description) by the structure of the space-time manifold. When this structure is clearly characterized the direction and rate of movement can be deduced in terms of a logical necessity. In general, the behavior of objects within a field may depend also on the character of the objects themselves. This is strikingly so of the behavior of objects in the electromagnetic field. The microscopic field structure of the objects, however, may enable us to deduce these characteristics.

How far is Freudian psychoanalysis to be reckoned as "class" or as "field" theory in this respect? Healy, Bronner, and Bowers[5] in their excellent recent work on Freud, state that one of the "foundation stones" of psychoanalysis is the fact that: "The essential nature of the individual consists in strivings and urges, innate or unlearned, which originally are quite independent of environment" (p. XX). Furthermore, certain of these urges, for instance, the newly discovered "aggressiveness" are determined also in their direction by the original nature of man. Freud

[5] The Structure and Meaning of Psychoanalysis, New York, 1931.

says, "aggression is an innate, independent, instinctual disposition in man,"[6] and suggests in innumerable passages in his recent works that this aggression is part of a general "Death instinct" which is directed towards other men. Besides the Death instinct, there is, of course, also the Love instinct (Eros) which is likewise innate and original. Such concepts as instinct, when used in this way, are "class" concepts. The whole idea of original human nature is a "class" concept. Certain behavior is determined by the class, in this case man, to which the object under consideration belongs.

If Freud's theory of instincts may be analyzed as "class" theory, other Freudian forces appear on first view to be more related to "field" theory. Since the genesis of ego and superego out of id[7] depend on experience, one might suppose that the activity of these parts of the ego was determined by the structure of the psycho-biological field. Not so, however. After they are developed, the behavior of these parts of the ego is determined in the individual and are part and parcel of the individual. For instance, "every impulse of aggression, which we omit to gratify is taken over by the superego and goes to heighten its aggressiveness against the ego." (Civilization and its Discontents, p. 114.)

2. *The force directing behavior shows the properties of an entelechy.*	2. *The force directing behavior shows the properties of a vector.*

Bodies move, according to Aristotle, because each harbors an "entelechy" which strives to return the body to its "rightful" place. The entelechy as Lewin pointed out, is in itself determined by the class to which an object belongs. In modern kinetics, the movement is defined by a vector whose direction and magnitude at any place are both determined by field structure. A stone without a field according to "field" theory would not behave at all, according to "class" theory it would presumably start looking for other stones.

Freud's speculations concerning the nature of forces acting in

[6] Civilization and its Discontents, p. 102.

[7] The Ego and the Id, 1923 and in the most of the subsequent writings; most recently, New Introductory Lectures, Lecture XXXI.

humans are "class" theory. The libido, which is the energy of the love instinct and is hence the chief driving force in humans, is an entelechy rather than a vector.[8] It is psychobiological energy resident within the body which strives through certain stages (infant sexuality, latency, adolescent sexuality) to the normal heterosexual fixations of adulthood. Even if it varies in intensity and strives under different conditions after different objects, these changes are looked on as accidental. It belongs to the original nature of man and its direction and magnitude are conditioned internally.[9]

3. *There is local determination.*	3. *There is no local determination.*

The return of the stone to the earth is determined "in" the stone according to Aristotle. Outside or accidental forces displace the stone, then the entelechy begins to act and it strives to return to its rightful place. In modern kinetics, there is no local determination. This should be clear from the above.

From our discussion of Freud's instinct theory and his theory of the behavior of ego, id, and superego, it should be clear that the strivings of mankind are locally determined. The working out of the Freudian mechanisms like repression and conversion show also that Freud is thinking in terms of class theory. The repressed idea like the stone of Aristotle strives to return to its "rightful" place, i.e. the consciousness.

4. *The concepts used in "class" theory are primarily substantial.*	4. *The concepts used in "field" theory are primarily functional.*

This distinction was first clearly formulated by E. Cassirer.[10] Aristotle's analysis is concerned with the substance of which the

[8] Healy, Bronner, Bowers, pp. 2–6.

[9] In fairness, it must be said that Freud himself realizes the inadequacy of his theory of urges. In the New Lectures (p. 131) he says: "Die Trieblehre ist sozusagen unsere Mythologie. Die Triebe sind mythische Wesen, grossartig in ihrer Unbestimmtheit.

[10] E. Cassirer, Substanzbegriff und Funktionsbegriff, Berlin, 1910. Under substantial and functional Cassirer also includes or implies most of the other criteria which I set up.

object is made. The substance of which the object is made is a matter of complete indifference to the field theorist. Freud uses concepts of both classes rather indiscriminately. The Freudian mechanisms (repression, conversion, etc.) are concerned with functions, but libido, ego, and superego are to be classified as substantial.

5. *The method of scientific analysis is primarily structural.*	5. *The method of scientific analysis is primarily functional (relational).*

The problem for the Aristotelian physicist to determine is of what the object is made. If it is a stone, then it is earth, and when thrown in the air moves downward. Modern kinetics was developed by varying the conditions under which movement occurs. The modern physicist always expresses his analyses in series of relationships.[11]

Psychoanalysis here again uses both procedures. The analysis is structural in so far as it attempts to uncover a series of complexes in the individual, and functional in so far as it attempts to relate these complexes to experience. Psychoanalysis is also using functional analysis in its attempts to create transference, sublimation, and the like. The question of the actual technique of analysis is, as I pointed out above, a problem with which I am not competent to deal. The historical development of the technique in the hands of Freud and his most important followers presents peculiarities, however, which suggest that one could profitably consider it more of a diagnostic art, than a scientific method.

6. *The analysis is in terms of historically and geographically conditioned regularities.*[12]	6. *The analysis is in terms of a historical-typical laws.*[12]

Aristotle as a physicist is concerned with empirical observations of his own time. Common experience of falling bodies in the

[11] J. F. Brown, A Methodological Consideration of the Problem of Psychometrics. *Erkenntnis*, 1934, (in print).

[12] *Cf.* Lewin, The Conflict, etc., op. cit.

atmosphere agrees better with Aristotle's physics than it does with the modern theory of the gravitational field. Every young student of physics is a doubting Thomas when he first hears about the piece of lead and the feather in the vacuum. With the exception of the movement of the heavenly bodies, Aristotle was content to accept the statistically regular as the lawful.[12]

Statements about falling bodies in modern kinetics are statements about *types* of events which hold throughout space and time. They are statements about logical constructs which physicists have postulated as underlying the given immediate experience. The field theorist *orders* his experiences to these logical constructs. In so far as his phenotypical experiences may be so ordered to these genotypical constructs that he may deduce what his subsequent experiences will be, he considers his scientific analysis successful.[13] The relationship between time and space expressed in the law of falling bodies, although never agreeing precisely with actual data, presumably holds for all past and future historical times and for all positions in the cosmos. Explanation of an event consists in adequately describing the underlying genotype and in seeing if the phenotype (experience or data) may be precisely *ordered* to it. Laws are descriptions of genotypes.

It is here that "field" theory enjoys the greatest advantage over "class" theory. When the laws (genotypes, underlying dynamics) are well enough known to allow measurement, one can predict the future quite independently of the past. That the climate of the past, soil erosion in past years, the date of the spring thaw, etc., etc., all have some part in determining the time at which an individual apple will fall is obvious. But the contemporary physicist, if he could make certain measurements, could predict the fall of the apple and where it would land with a high degree of accuracy quite without reference to its past

[13] *Ordering* phenotypical experience to genotypical constructs (Lewin) may also be expressed (with some reservations, *cf.* above) as *translating* "Protokolsprache" into "Physikalischesprache" (Carnap) or as *translating* the language of data into the language of constructs (Feigl). *Cf.* the references given above to these writers. Phenotypes, according to Lewin, represent experience; genotypes, the underlying dynamic situation. *Cf.* Lewin, Gesetz und Experiment in der Psychologie, Berlin-Schlachtensee, 1927.

history. He might characterize the fall with a single vector. He needs no knowledge of the history of the tree. The fact that there are many contradictory theories of cosmic evolution is a matter of indifference to the physicist. The psychologist always excuses his lack of precision by referring to the difficulties of a genetic nature which hamper him.[14]

Healy, Bronner, and Bowers point out that "whatever an individual is or does at any given moment is largely predetermined by his earlier experiences and his reactions to them" is a basic postulate of Freudian psychoanalysis.[15] Consequently, psychoanalysis uses historical rather than ahistorical analysis. No one in his senses would question Freud on the truth of this point, any more than he would question the importance of the past history of the tree in connection with the fall of the apple. Whether a theory of the psychobiological field can be set up which will function in the same way as modern kinetics does with regard to past history is an open question. Psychoanalysis is not such a theory. Psychoanalysis is forced to "explain" present behavior as "caused" by events in the remote past. Furthermore, the indiscrimate mixture of phenotypical descriptions (language of data) with genotypical descriptions (language of constructs) is commonplace in the existing psychoanalytical theory. One example must suffice. Past *experiences* are said to affect present *physical states*, for instance, in the conversion of repressed libidinal urges into the symptoms of hysteria.

7. *The method is primarily empirical.*	7. *The method is hypothetical-deductive.*

This point is closely related to the foregoing. Aristotle's descriptions fitted his experiences fairly accurately. His experience (without control or verification through the experimental method) dictated his theories. Perusal of Galileo's "Discorsi" convinces one that Galileo deduced the relationship

[14] *Cf.* K. Lewin, Die Grundlagen der dynamischen Psychologie, Leipzig, to be published in 1934. I am indebted to Prof. Lewin for allowing me the use of the manuscript of his book.

[15] Op. cit., p. xx.

between time and space, before he performed his justly famous critical experiments on the inclined plane. In field-theoretical analyses all experiments are critical, i.e., they are performed to test the validity of rather precise working hypotheses. The hypotheses themselves require an adequate definition in terms of the original concepts and the experimental situation must be adequate to decide on the validity of the hypotheses. Working hypotheses then precede experimentation. As H. Dingler[16] has pointed out, statements like $s = \frac{gt^2}{2}$ cannot possibly be verified purely inductively. When, however, under increasingly well controlled conditions, the data more and more approximate the law (or working hypothesis) we consider it verified.

Freud has emphasized the empirical character of his findings throughout his scientific career. On the basis of these empirical findings the vast structure of psycho-analysis has been erected. The theory has always been sufficiently elastic to be varied with the acquisition of new facts. But, and here *almost all* critics of psychoanalysis are in agreement, *the theory has never been precise enough to allow formulation of working hypotheses for which adequate experimental situations could be found.* In his latest book[17] Freud himself seems to see this inadequacy. He hopes in the future to see an experimental approach and reports as a beginning experiments performed by Schrötter in connection with dream symbolism and others performed by Betlheim and Hartmann on Korsakoff's Psychosis. Lack of space forbids a detailed account of these experiments. They represent an attack in the right direction, but I believe few scientists would consider them critical experiments.

8. *The analysis allows dichotomies.*	8. *The analysis allows no dichotomies.*

Only in the sphere of the ether are absolutely regular laws to be found, according to Aristotle. The only laws which brook no

[16] H. Dingler, Der Zusammenbruch der Wissenschaft, etc., Munich, 1926. *Cf.* also J. F. Brown, op. cit.

[17] New Introductory Lectures.

exception are those of astrophysics. In the theory of the gravitational field, on the other hand, there is no essential difference between events on the earth and events on the moon.

In this connection Freud might serve as an example of the field theoretical mode of thinking. He, more than any single psychologist, has furthered the idea that abnormal mental phenomena differ from the normal in degree and not in kind. Freud was the first to insist that the symptoms of mental disease were to be looked on as exaggerations or perversions of the normal mental phenomena.

9. *"Class" theory tends to use valuative concepts.*

9. *"Field" theory insists on non-valuative concepts.*

Movements in the ether were on a "higher" plane (i.e. showed perfection), while movement on the earth was "imperfect" according to Aristotle. The approach of modern science, as has been pointed out time and time again, is concerned with neither the good nor the bad, neither the beautiful nor the ugly. On this point, particularly in his completely scientific attitude towards the manifestation of the sexual impulses, Freud has aligned himself with the best scientific tradition. His coolness, his disinterestedness, and his refusal to compromise his position in his fight against the prejudice of little minds may well become an example for future generations of biological scientists.[18]

10. *Class theory attempts to answer a metaphysical "why?".*

10. *Field theory attempts to answer a scientific "how?".*

Aristotle was trying to answer the question: "Why do bodies move?" Galileo, the first thinker in the field-theoretical tradition, attempted the more modest question: "How do bodies move?" I believe it could be shown that the question "why?" as posed by Aristotle must imply a metaphysical answer. When-

[18] *Cf.* his own description of his early struggle in, "A History of the Psychoanalytical Movement," New York, 1916.

ever one poses questions like "why movement?", "why life?", "why love?" one must begin to think in terms of animism or "class" theory. To a certain extent "field" theories also answer the question "why?", but it is a different sort of "why?". It is, I believe, synonomous with "how?" Field theory, as we have seen above, shows the logical necessity of connection between events by ordering phenotypical or observed data to genotypical constructs. Science never explains why nature is the way it is, but simply relates certain events to others in terms of logical necessity. It doesn't even answer *why* they should be related at all, but simply shows *how* they are related. Explanation in science is description in a language which allows us to derive certain necessary implications from certain concrete cases.

Freud here attempts answers to both types of question. When he is concerned with the relationships between abnormal mental phenomena and human desires he is concerned with the question "how?". But he is metaphysical in the worst sense when he attempts, as he does, to answer the questions "why life?", "why love?", "why death?". Questions of this nature in themselves may only be answered in metaphysical terms.[19]

Where then does Freud stand in the light of our ideas of what constitutes "good" and "bad" scientific theory? He represents a curious mixture of both types. He is strikingly a "good" theorist in his lack of dichotomies and in his lack of valuative concepts. On the other hand, the forces postulated by Freud to account for human behavior are largely determined by "class"; in place of vectorial forces he uses entelechies; behavior is locally determined; his analyses are in terms of historical-geographical regularities rather than in ahistorical-typical laws. His method shows indiscriminate use of both structural and functional analysis; his concepts are partly substantial, partly functional; he uses the hypothetical-deductive method but only inadequately; and he fails to distinguish between metaphysical "why" and scientific "how."

[19] *Cf.* "Civilization and its Discontents," and "The Future of an Illusion" for striking examples of this point.

This has been a critical paper and consequently we have not stressed Freud's positive contributions. One of the best ways of damning a scientific development with faint praise is to say, "Here we have a promising beginning." Freud's psychoanalysis represents very much more than a promising beginning. When McDougall calls Freud the "greatest psychologist since Aristotle" he is not indulging in meaningless flattery.[20] Freud's discoveries probably are the most striking and original contributions made to the science of mind in our time. He is, however, more in the prophetic than in the scientific tradition. If I may speak in terms of "vergleichende Wissenschaftslehre" again, Freud seems destined to be psychology's Bruno rather than her Galileo.

The reader may ask: Is there any use in making such a study of Freud as you have made? Is there any hope that psychology may ever be written in terms of a field theory? I believe these last two questions may be answered with an emphatic yes. Beginnings have already been made towards a psychological approach in terms of field theory. The field theory *cannot* yet handle many of the problems, which psychoanalysis attempts to handle. Where it *is* applicable, however, it *does show* the advantages we have spoken of. A more detailed discussion of the field theory lies without the limits set for this paper. My readers are hence referred to the literature.[21]

It is further to be hoped that methodological criticism, such as this, will prevail on the psychoanalysts themselves to examine some of their basic concepts. It must be emphasized that Freud has never looked on psychoanalytical theory as rigid, that he has always been eager to adjust the theory to new data, although he has always looked as well for new data to support the many speculations of his fruitful mind. We have seen above that he has recently become interested in the possibility of experimental verification of his hypotheses. Before critical experiments may

[20] McDougall, op. cit. It is interesting that McDougall's use of purpose, Freud's use of libido, and Aristotle's entelechy are all "class" concepts, however.

[21] See the forthcoming book of Lewin, "Die Grundlagen der dynamischen Psychologie" and the series of papers by him and his students appearing in *Psychol. Forsch.* I hope in the near future to publish some studies on problems of psychopathology, which show the applicability of the field theory to abnormal as well as normal mental phenomena.

be performed, working hypotheses must be set up, concepts must be precisely defined. I am so bold as to suggest that the psychoanalysts give the precise definition of their concepts more attention.

In this paper I believe to have shown:

(1) That it is possible to set up criteria to distinguish between "class" theories and "field" theories. Aristotle's kinetics fufils all the criteria for "class" theory, modern kinetics fulfils all the criteria for "field" theory.

(2) Freud's psychoanalysis fulfils some of the criteria for "field" theory, some for "class" theory, and with regard to some criteria is a mixture of the two types of theory.

(3) Freudian theory could profit through reexamination of some of its basic postulates. This will be particularly necessary if the clinical findings of the analysts are to be supported by an experimental approach; as Freud has recently suggested. I have suggested:

(1) That some, at least, of Freud's discoveries might be explained in the framework of a "field" theory.

(2) That the postulates of a "field" theory are probably in better agreement with psychological reality than certain postulates of the Freudian theory in its present form.

Psychological Laboratory,
University of Kansas,
Lawrence, Kansas.

Scientific Method and Social Science

BY

JOSEPH MAYER

IF THERE is an essential difference as suggested in a preceding article,[1] between the natural sciences on the one hand and the social studies on the other, in the sense that man has the power to change, and has repeatedly changed, existing social organizations, whereas he has no such power over natural phenomena, the meaning of social science must in this respect at least differ substantially from that of natural science. Elsewhere the present writer has designated society an "artificial creation," much as the automobile or a steel mill is, i.e. made by the artifice of man.[2] The artificial automobile or steel mill is of course fabricated in the light of the natural laws of physical science. Its construction would be impossible without a knowledge of these laws. There are also certain natural laws and tendencies in human nature and animal behavior which must serve as a foundation for any scientifically fabricated society, i.e. constructed on the basis of observation and verification of these tendencies on the one hand and a rigorous use of theory

[1] Joseph Mayer, "Toward a Science of Society", *The American Journal of Sociology*, September, 1933, pp. 176–177.

[2] Joseph Mayer, "The Seven Seals of Science", *The Century Co.*, New York, 1927, pp. 424 ff. *Cf.*, also, Lester F. Ward, "Dynamic Sociology", *D. Appleton & Co.*, New York, 1883, vol. i, pp. 71–81, 490–493, vol. ii, pp. 103–106; "The Psychic Factors of Civilization", *Ginn & Co.*, Boston, 1893, pp. 286–7; "Applied Sociology", *Ginn & Co.*, Boston, 1906, pp. 11–12, etc. One need not agree with Ward's view of teleology or psychology (which in his day were very crudely conceived) to appreciate the distinctions he drew between natural and artificial in the foregoing references and elsewhere.

and analysis on the other. In short, despite any essential difference, as suggested, between natural and social science, the scientific method is apparently just as applicable in the one field as in the other. There is still much confusion on this score. But before pursuing further the significance of the distinction, artificial versus natural, it will be well to have before us more of what opposing schools of methodological thought in the social studies have to offer.

There are in fact quite a number of schools of social thought at the present time. For our purpose they readily divide themselves into two groups: a group of natural science schools and a group of cultural schools.[3]

CERTAIN TENDENCIES IN NATURAL SCIENCE SCHOOLS OF SOCIAL THOUGHT

The natural science schools are themselves of many varieties. In large part they reflect the scientific vogue and the advent of specialists from the mathematical disciplines and the physical and biological sciences, in the social field.[4] Where their rise implies a more rigid application of scientific method to social phenomena, an important advance step may be anticipated, but where there is implied an attempted reduction of social to physico-biological elements, with the consequent neglect of the social elements themselves, and a crass emphasis upon quantitative method to the exclusion of essential social factors that are uniquely qualitative in nature, opposing cultural schools find legitimate ground for criticism.

The terms most generally used by this group of schools are "natural," "quantitative," "objective," "behavior," and "sense impression," stressing, it will be noted, the observational and verification side of scientific method, rather than the rational and analytical.

Extremists of this group tend to reduce "natural" to "physical." Thus Durkheim speaks of social facts as "things." They

[3] *Cf.* Chas. A. Ellwood, "Methods in Sociology", *Duke University Press*, Durham, 1933; Pitirim Sorokin, "Contemporary Sociological Theories", *Harpers*, New York, 1928.

[4] Sorokin, *op. cit.*, Intro., chs. I, IV, etc.

emphasize "objective" or external data to the exclusion of "subjective" or mental data, such as feeling, belief, value, etc. Quantitative verification appears as one of their chief objectives, anything qualitative and non-mathematical being regarded as falling outside the scientific purview. Sense impressions are spoken of as mere "excitations" in an effort to escape "subjective" implications. And human "behavior," from which "introspection" is rigidly excluded, would seem to be reduced to physico-chemical or at best to purely physiological terms. Organisms tend to become "ingenious mechanisms," which develop through unconditioned reflexes, conditioned reflexes, and habitual responses. On the social plane such responses constitute a "collective reflexology," to use Bechtereff's designation.[5]

Some sociologists of the natural science schools will probably regard parts of the foregoing outline of extreme tendencies as exaggerations, built up by the opposition schools from isolated references to lend more color to their counter arguments. Nevertheless there is sufficient evidence to demonstrate their wide-spread existence.[6]

OPPOSING TENDENCIES AMONG CULTURAL SOCIOLOGISTS

In reacting against extreme natural science arguments, the cultural schools of social thought emphasize another side of scientific method, viz., the theoretical and analytical. To this extent they tend to restore a very important and proper balance in the application of scientific method to social study. But they likewise do much more. Some of them also go to extremes and often deny all validity to observational and testing methods in sociology and economics. It should in fact be noted that many partisans among the cultural sociologists draw their scholarly sustenance from non-scientific disciplines (from theology, practical social work, history, and philosophy), that the very terms used by the schools they oppose are often anathema to them, and that, to some extent, they represent the "second generation" of sociologists whereas extremists among the natural science

[5] *Cf.* Ellwood, *op. cit.*, introduction and first four chapters, especially pages 31–45.
[6] *Ibid.*; also, Sorokin, *op. cit.*

groups represent the "third."[7] It is presumably the revolt of the latter against the former which has resulted in the present methodological conflict in the social studies, a conflict which at its best is instructively reminiscent of the Bacon-Descartes controversy of the early seventeenth century and the Aristotle-Plato struggle of ancient times.[8]

Adherents to the cultural point of view insist that social phenomena are more amendable to philosophical than to natural science methods, that introspection and "subjective" analysis must continue to be of paramount importance, that social relations are externally observable to only a very slight extent, and that, in place of and in contrast to observational, experimental, and mensurative procedures, the chief methods of the social scientist must be imagination, psychological insight, and historical interpretation, with statistics, surveys, and case studies playing subsidiary rôles.[9]

Objections to behaviorism are along somewhat similar lines, viz.: it sets up an extreme orthodoxy which enslaves rather than frees the mind with respect to scientific experimentation; it is inadequate and dogmatic in denying "the existential reality of the non-physical" and thus cannot deal with the most important aspects of culture; it uses a pedantic and meaningless jargon in place of a direct and clear terminology; it fails to reveal the true nature of the social process, i.e., intercommunication of conscious and articulate experiences among human beings, and while it may be applicable below the human level it is not adequate on it; it cannot provide an understanding of adult human actions in their cultural aspects, which constitute the most important of individual and social phenomena; and it does not begin to penetrate into the meaning of social institutions with their emphasis upon values and the valuation processes.[10]

[7] Ellsworth Faris, Review of Ellwood's "Methods in Sociology", *The American Journal of Sociology*, March, 1934, pp. 686–689.

[8] Mayer, "Seven Seals of Science", pp. 13–14, 31–33, 98–106, 201–209.

[9] *Cf.* Ellwood, *op. cit.*, chs. II & V; also, Morris R. Cohen, "Reason and Nature", *Harcourt, Bruce & Co.*, New York, 1931, pp. 333–368; Chas. H. Cooley, "Life and the Student", *Alfred A. Knopf*, New York, 1927, pp. 150–158.

[10] *Cf.* Ellwood, *op. cit.*, ch. IV.

OBJECTIVE VS. SUBJECTIVE METHODOLOGY

The more moderate among the proponents of natural science methods in social study find little to criticize in such objections to extreme behaviorism as those indicated above, but with respect to the attempted subordination or replacement of observational and other "objective" methods, even they give no quarter.[11] The term "objective" nevertheless continues to be ambiguous, in the writings not only of sociologists but also of physicists, biologists, and even of philosophers.[12] Where it is simply used to designate the disinterested, strictly impartial, detached search for truth in sociology as elsewhere, no one can conceivably find fault.[13] It is its logical, psychological, and philosophical implications which not only continue to create difficulties but, in the light of recent developments in physics, would seem to render the difficulties more profound today than at any previous time.

From one point of view the quarrel between the natural science and cultural groups is mainly over the relative significance of the "objective" and the "subjective," of "observation" and "introspection," and insofar as this is true the quarrel is not easily settled. It may not be settled for years to come. It is in this respect that the present situation appears to be the revival of an old dispute, begun by Plato and Aristotle, opened anew by Descartes and Bacon in the light of seventeenth century physical evidence, and apparently now being renewed again as a result of twentieth century physical advance.

The old syllogistic logic settled the question too easily. Nature was regarded as a pack of shuffled facts, and all the scientist had to do was to go out, gather an armful, and then in his study arrange them neatly into their proper fore-ordained suits. But the logic of the scientific renaissance destroyed all that. Scientific method took on a "question and answer" character. The scientist asks nature a question. If it is crude or raw, he gets a crude or raw answer. If then he rephrases and improves his

[11] *Cf.* Faris, *op. cit.*, p. 689.

[12] *Cf.* Arthur O. Lovejoy, "The Revolt Against Dualism", *Open Court Publishing Co.*, United States, 1930.

[13] *Cf.* Ellwood, *op. cit.*, pp. 28 ff.; also, Faris, *op. cit.*, p. 688.

question, he gets a better answer. And so on. Facts (observation) can no longer be regarded as uncolored by the question asked (theory), nor is the scientific question uncolored by preceding facts. The "objective" and the "subjective" are thus much more closely related than had previously been perceived.

Similar reasoning applies to the psychological approach. In talking of "excitations," certain extreme behaviorists are harking back to a day in the history of psychological development which is long since gone. There are no "pure" excitations or sensations, uncolored by introspective elements. Rigorous introspection may, in fact, now be regarded as a refinement of cruder "objective" observation.[14] The really important consideration here, for the psychologist and sociologist, is the matter of devising suitable techniques of testing and verifying introspective observation, a much more difficult problem than the simpler quantitative tests applied to ordinary "objective" observation.[15]

From the philosophical point of view, the problem remains one of psychophysical dualism of mind and matter or epistemological

[14] Mayer, "Seven Seals of Science", pp. 207–210. For further details regarding the significance and importance of introspective- or self-observation, see works of E. B. Titchener; also additional references given on pages 718–719 of "Methods in Social Science", edited by Stuart A. Rice, *University of Chicago Press*, Chicago, 1931. Certain "modern" behaviorists, who attempt to drag introspection through the window after it has been so roundly kicked out the front door, are now designating introspection as reflective or symbolic "behavior", adding that they see no objection to introspective- or self-observation so long as knowledge gained thereby is duly verified and checked by other self-observers,—a point on which there has never been any question. The real question has to do with the attempt to add "behaviorless" behavior of a reflective or symbolic sort to honest-to-goodness behavior and expect to retain the dogmas of behaviorism intact. Under the expanded concept, an organism is apparently never "not behaving"; and so the "moderns", in their return to Wundt and his followers, seem not only to have taken the "ism" out of behaviorism (an avowed intention) but to have taken out "behavior" as well. *Cf.* John F. Markey, "Trends in Social Psychology", in *Trends in American Sociology*, edited by Lundberg, Anderson, Bain, *et. al.*, *Harpers*, 1929, pp. 135–148; J. F. Dashiell, "A Physiological-Behavioristic Description of Thinking", *Psychological Review*, 1925, pp. 54–73; K. S. Lashley, "The Behavioristic Interpretation of Consciousness", *Psychological Review*, 1923, pp. 237–272, 329–353; A. P. Weiss, "A Theoretical Basis of Human Behavior", *R. G. Adams & Co.*, Columbus, 1925, pp. 234 ff.

[15] For the development of techniques applying to the precise study of the "higher mental processes", see the excellent analyses, by J. E. Coover and Franklin Fearing, of the pioneer work of H. Ebbinghaus and of the Würzburg Laboratory, in "Methods in Social Science", edited by Rice, pp. 707–728.

dualism of representative (or symbolic) perception, or both, so admirably analyzed and summarized in Lovejoy's recent book, in which it is pointed out that the revolt of the past two centuries, as exemplified in the rise of pragmatism, instrumentalism, objective relativism, and such theories as Whitehead's and Russell's, has failed and that philosophy is face to face again with an age-old problem but in a new form because of recent advances in physical science.[15a] "Subjective" and "objective" appear, more reasonably, as the "inside" and "outside" of the same reality, each of which seems equally indispensible to the proper understanding of that reality.[16]

Observation lost a great deal of its seemingly absolute objective character after physical discoveries in sound and color forced the recognition of a differentiation between so-called "primary" and "secondary" qualities in matter.[17] Extension, form, and motion continued to be regarded as "objective," but color, sound, taste, etc., took on decidedly "subjective" aspects. Since seeing, hearing, touching, tasting, or smelling *is* observing, it came to be appreciated that observation has subjective as well as objective characteristics,[18] and that the uncritical observation of the physical and biological sciences remains raw and crude unless or until corrected by what may legitimately be designated as more rigorous introspective observational techniques developed by psychological analysis.

Nevertheless the die-hards among the objectivists continued to hold fast to extension, form, and motion, as the primary and objective elements of the material universe,—elements which, they held, are quantitatively measurable, absolutely invariable, and form the basis of all science, uncolored by subjective considerations. Then came the revolutionary discoveries of the 90's bearing upon atomic structure and electronic behavior, with respect to which the relativity and quantum theories were later

[15a] Lovejoy, *op. cit.*

[16] *Cf.* Cooley, "Life and the Student", p. 151; also, Mayer, "Seven Seals of Science", pp. 206, 386–387.

[17] Mayer, "Seven Seals of Science", p. 203.

[18] *Ibid.*, pp. 208–209.

developed and as a result of which extension, form, and motion also lost much of their "objective" aspects. Smoothness, compactness, impenetrability, became more seeming than real and all our ideas of matter, energy, space, and time have had to be recast in more "subjective" molds.[19]

And now comes the so-called "principle of uncertainty," enunciated by Heisenberg in 1927, which, had it been put forth by a philosopher or a sociologist, would probably seem utterly fantastic. Developed to reconcile conflicting theories in modern physics (largely to explain the wave and particle aspects of electronic behavior), this new principle not only demonstrates the uncertainty and subjective character of fundamental knowledge of the atomic universe but also indicates that such knowledge must always remain uncertain, inexact, probable, and statistical because of inherent limitations in the observational process. For example, to "see" an electron requires light; but to illuminate an electron gives it added characteristics; so that the very act of attempting to observe it in a given state makes the observation of that state uncertain or impossible. Hence the electron appears to present itself in two aspects, one as a particle and the other as a wave, both equally elusive; but observation offers no method for resolving or getting behind these appearances and so physics must content itself with accepting the conflicting illusions as the best it can probably achieve and as "equivalent" so far as any space-time description is concerned.[20]

Absolute objectivity has therefore virtually become a meaningless concept. If seventeenth century physical advance tainted it with subjectivity and nineteenth century developments destroyed its absoluteness and made all physical knowledge relative to the observer (including space, time, and cause), twentieth century progress has gone even further and has indicated that the "object" of observation and the "subject" observer con-

[19] *Ibid.*, pp. 253–264.

[20] *Cf.* R. B. Lindsay, "Some Philosophical Aspects of Recent Atomic Theory", *Scientific Monthly*, April 1928, pp. 299–305; J. Rud Nielsen, "Philosophical Implications of Modern Physical Science", *Scientific Monthly*, June, 1931, pp. 546–555; C. G. Darwin, "The Uncertainty Principle in Modern Physics", *Scientific Monthly*, May, 1932, pp. 387–396.

stantly interact, that there are very definite limits to observational knowledge making for uncertainty and illusion, and that scientific facts are probably just as much "subjective" as "objective."

Such a conclusion does not mean that the distinction between the object and the subject has disappeared. Observation of the objective universe still remains the basis of all scientific knowledge. But it does mean that the development of observational techniques is probably only in its infancy, that subjective factors can no longer be naïvely disregarded, that quantitative measurement is evidently the simplest and crudest of observational devices, and that the emphasis in psychology and the social studies must be more and more upon the perfection of instruments which will register, record, and classify qualitative distinctions and upon providing tests and verification procedures for those intangible though very real elements in human nature which are preëminently mental and social.[21]

Nor does the foregoing analysis imply any change in the character of scientific method, which remains observation and testing, on the one hand, and theory and analysis on the other. It is simply no longer possible to regard "objective" and "subjective" as respectively synonymous with these two scientific processes or with "impartial" and "biased" procedures. Both observation and theory should be as "objective" as possible in the general sense used at the beginning of this section, i.e., they should be disinterested, impartial, detached. Both, however, contain "subjective" elements which are inherent though different in each.[21a]

[21] *Cf.* forthcoming article by present writer entitled, "Comparative Value and Human Behavior".

[21a] There is another meaning sometimes proposed for "objective", viz., that which would have the concept coincide with the abstracting operation in physical and biological science, by means of which "irrelevant" factors (such as the influence of the air on a falling body) are omitted in controlled experiments (as in a vacuum). But a controlled experiment does not take nature as the observer finds it, in all its pristine "objectivity". Experimental "abstraction" is a decidedly "subjective" operation, even though it is admittedly one of the most important refinements of scientific method in bringing to light the invariants of nature. See L. L. Bernard "The Objective Viewpoint in Sociology", *American Journal of Sociology*, November, 1919, pp. 298–325 (307). Attempts to substitute explicit-implicit for objective-subjective obviously solve none of the problems outlined in this section. See Floyd H. Allport and Dale A. Hartman, in "Methods in Social Science", edited by Rice, pp. 308 ff, 345 ff.

Finally, it should be repeated that the scientific method of observation and theory remains the same whether applied to the study of physical, mental, or artificial phenomena, whether to an investigation of the moon, a panic, or the kitchen clock. The young hopeful may take the clock apart and observe its elements ever so painstakingly, but if he has no adequate theory as to its interrelations he will never find out what makes the wheels go round nor in what its uniqueness as a whole consists. Whether society is physical, mental, natural, or artificial does not change the approach to its scientific understanding. But, on the other hand, no attempted reduction of its intangible interrelations and values to physical or physiological terms, be their observation and quantitative appraisal ever so carefully carried out, will bring us one whit nearer either to the observation and testing of all-important cultural aspects which are thus omitted or to a more rigorous theoretical analysis of basic social and economic variables.

ESSENTIAL CONTRIBUTIONS OF NATURAL SCIENCE AND CULTURAL SCHOOLS OF SOCIAL THOUGHT

We may now return to the schools of social thought already partially reviewed and attempt to summarize very broadly their respective contributions to sociological methodology.[22]

The natural science groups emphasize the fundamental need for observation and verification in social study as in natural science. The cultural groups point to the necessity of adequate theories and analyses here as elsewhere in science and, particularly, of dealing with distinctive mental and social facts rather than with physical, chemical, and biological material which belongs primarily to other sciences.

The natural science groups emphasize the virtue of applying quantitative measurement to social study, of developing statistical and other mathematical techniques for presenting social facts in verifiable form. The cultural groups point to the pri-

[22] A recent publication giving details with respect to the natural science point of view is: "Trends in American Sociology", edited by Lundberg, Bain, Anderson, and others, *Harpers*, New York, 1929. For the cultural point of view, see Charles A. Ellwood, "Methods in Sociology", *Duke University Press*, Durham, 1933. *Cf.*, also, "Methods in Social Science", edited by Stuart A. Rice, *University of Chicago Press*, Chicago, 1931.

mary need for developing methods of impartial evaluation of qualitative cultural factors involving ideals, values, and ethical considerations.

The natural science groups emphasize the importance, for cultural analysis, of gathering data regarding behavioristic patterns of social response found in man viewed from the "outside," in children, in primitive peoples, in the other primates, and in other animals which have gregarious habits or customs. The cultural groups aim to deal directly with adult human behavior from the "inside" and with cultural institutions as such, insisting that here are to be found the basic variables for a science of society and that these cannot be understood except by methods which are competent to analyze such "subjective" factors as ideas, emotions, volitions; traits, impulses, wishes, desires, motives; values and other ethical concepts.

THE NATURAL VS. THE ARTIFICIAL IN CULTURE

Culture is made up of diverse elements: material objects, such as tools, instruments, machines and other physical products of the artifice of man; individual skills, techniques, crafts, trades, professions, etc.; institutions and customs, i.e., the traditional ways and social conventions to which people conform.[23] Certain cultural elements are natural or merely divert natural forces for more effective human use. A great part of culture, however, is fabricated; it is artificial; it results from the artifice of man.

Among the major problems of social science would seem to be to determine: (1) in what respects culture is inherently natural and subject to almost no control by man; (2) to what extent it is natural but clearly divertible into channels in keeping with man's needs or desires; (3) to what extent it is artificial and thus

[23] *Cf.* Edward B. Tylor, "Primitive Culture", *John Murray*, London, 1871, 2 vols.; A. A. Goldenweiser, "History, Psychology and Culture", *Jour. Philos. Psycho. and Sci. Method*, October, 1918, pp. 561–571, 589–607; Clark Wissler, "Man and Culture", *T. Y. Crowell*, New York, 1923; M. J. Herskovits and M. M. Willey, "The Cultural Approach to Sociology", *American Journal of Sociology*, Sept., 1923, pp. 189–199; Malcolm M. Willey, "Society and its Cultural Heritage", in *An Introduction to Sociology*, edited by Davis and Barnes, *D. C. Heath*, New York, 1927; Joseph Mayer, "Review of a Guide to Historical Literature", *Isis*, April, 1932, pp. 459 ff.

completely changeable and controllable by man. Insofar as society is an artificial creation, the problem of social change or planned control becomes understandable. Otherwise it would be just as unthinkable to speak of such a possibility as it would be to talk of man's planning out new movements for the planets.

Neither the natural science nor the cultural schools appear to appreciate sufficiently this distinction between the natural and the artificial in cultural relations, though the distinction would seem to be implicit in otherwise inconsistent elements in their respective points of views.[24]

Whatever is wholly natural is presumably inevitable, such as organic evolution. Faris points out that the younger sociologists of the third generation take "the position that sociology is a natural science." Yet he also states that they doubt "the validity of the concept of evolution as applied to human society" and are dissatisfied "with the notion of any inevitable progress."[25] Surely the doubt and the dissatisfaction are inconsistent with the view that sociology deals with nothing more than natural phenomena.

Ellwood's statement that "the social sciences are much more sciences of culture than of nature" would seem to distinguish the cultural from the natural; and in his discussion of a scientific basis for ethics he presents certain considerations which the natural science sociologists apparently overlook.[26] At the same

[24] *Cf.* W. F. Ogburn, "Social Change", *B. W. Huebsch*, New York, 1922; A. A. Goldenweiser, "Cultural Anthropology", in "The History and Prospects of the Social Sciences", H. E. Barnes, ed., *Knopf*, New York, 1925; F. S. Chapin, "Cultural Change", *Century Co.*, New York, 1928.

[25] Faris, *op. cit.*, p. 687.

[26] *Ibid.* The natural-science sociologists rightly react against moral pronouncements that are "presumptuous" and "pontifical"; but they cannot thus evade the issue that, in any "planned control" or "new deal" changes, some "standard" or "norm" is implied. See F. H. Giddings, "The Ethics of Social Progress", *International Journal of Ethics* January, 1893, pp. 137–164; A. J. Todd, "Theories of Social Progress", *Macmillan*, New York, 1918; L. L. Bernard, "The Conditions of Social Progress", *American Journal of Sociology*, July, 1922, pp. 21–48; also his "The Development of the Concept of Progress", *Social Forces*, January, 1925, pp. 207–212; May, 1925, pp. 617–622; Sept., 1925, pp. 36–43; L. T. Hobhouse, "Social Development", *Allen & Unwin*, London, 1924; E. Sapir, "Culture, Genuine and Spurious", *American Journal of Sociology*, January, 1924, pp. 401–417; U. G. Weatherly, "Social Progress", *J. B. Lippincott*, Philadelphia, 1926; C. A. Ellwood,

time cultural sociologists often talk about social or cultural "evolution" or about institutional changes that are, it would seem, historically fore-ordained.[27] Studies of the artificial aspects of culture should help to clarify some issues about which controversy now so violently rages.

Regardless of the question of artificiality, however, it would seem that any scientific social reconstruction or control should follow *after* a clear understanding of the natural elements in social behavior and underlying social behavior. It is undoubtedly true that these elements can be properly comprehended only through a rigorous scientific methodology. Observation and verification; theory and rigorous analysis:—these must continue to be applied in the social and economic fields as elsewhere if a veritable social science is ever to be forthcoming.[28]

Library of Congress,
Washington, D. C.

"Cultural Evolution", *Century Co.*, New York, 1927; J. K. Folsom, "Culture and Social Progress", *Longmans Green*, New York, 1928; J. O. Hertzler, "Social Progress", *Century Co.*, New York, 1928.

[27] *Cf.* Ellwood, "Methods in Sociology", pp. 81, 128; also most of the references in footnote 26.

[28] For further references with respect to scientific method, see: Frederick Barry, "The Scientific Habit of Thought", *Columbia University Press*, New York, 1927; A. d'Abro, "The Evolution of Scientific Thought from Newton to Einstein", *Boni & Liveright*, New York, 1927; Joseph Mayer, "The Seven Seals of Science", *Century Co.*, New York, 1927; Jacques Rueff, "From the Physical to the Social Sciences", *The Johns Hopkins Press*, Baltimore, 1929.

DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

THE CHEMISTRY OF TIME

Sir:

In connection with Dr. Reiser's discussion of the physiological basis for the sense of time, it might be worthwhile pointing out that the writer has recently studied estimations of duration as a function of temperature in patients suffering from fever, and in those in whom high body temperatures were induced by diathermy treatments. This work was suggested by studies of rhythmic activities in cells of the central nervous system of animals, apparently produced spontaneously by chemical changes going on continuously within the cells. It seemed possible that one's estimation of time might depend upon continuous chemical changes within cells of the brain and that psycho-physiological time might thus be inversely proportional to the velocity constant of one of these changes.

In a series of irreversible reactions the slowest of the series may serve as a "master reaction" or pacemaker reaction. In such a system temperature would be expected to modify the end result, in this case estimation of time, in accordance with the Arrhenius equation relating velocity of chemical reactions to temperature. In addition it might be possible to obtain a value of the critical thermal increment from this equation which would be indicative of the possible catalytic mechanism controlling the speed of the master reaction.

The subjects were asked to count to sixty at a rate of what they believed to be one per second. The speed of counting not only obeyed the Arrhenius equation, but yielded, in a number of cases, a critical thermal increment of 24,000 calories, a value which previous workers have found, in certain cases, to be associated with respiratory phenomena in cells.

This result is hardly consistent with the notion that our sense of time is produced by the phenomenon of summation of processes initiated by

external stimuli, although these stimuli may well modify the speed of our basic chemical clock. Such modifications would account for the difference between judgments of filled and empty time.

The writer has considered these problems at greater length elsewhere.[1]

HUDSON HOAGLAND.

Department of Biology,
Clark University.

Sir:

Dr. Hoagland's suggestion that the physiological basis for our sense of time is related to an underlying chemical "master reaction" in the cells of the brain and that judgments of duration therefore should vary with modifications of the internal body temperatures, in a way consistent with the Arrhenius equation, is very interesting. I am sorry his paper was not available when my own was written, but my general thesis, based on the idea that the unidirectionality of psychic time has as its biochemical basis the irreversibility of the second law of thermodynamics, would not need to be modified in order to include Dr. Hoagland's data. For the benefit of those who, like myself several years ago, may wonder how the notion of the "master reaction," which is the *slowest* reaction acting as "pace-maker," fits in with Dr. Charles M. Child's conception of physiological gradients (regions of *highest* metabolic rate which dominate regions of lesser metabolic rate) it may be said that there is no real contradiction.

There are, however, several differences in emphasis in Dr. Hoagland's and my own version. In the first place, I should want to assign a greater importance to sensory control over the chemical clock than Dr. Hoagland's seems to recognize, for otherwise I see no way of explaining the very rapid passage of time which occurs in some dreams. Secondly, it should be recognized (what Dr. Hoagland admits, but does not sufficiently emphasize) that the conditions of his experiment presuppose the constancy of other variables than temperature, and that in reality other factors also help to determine the reaction velocity and time sense. I do not mean merely such parameters as concentrations, pressure, etc., but even more general considerations. If it is true, as Einstein holds, that a gravitational field slows down the vibrations of atoms (causing a shift of the spectral lines towards the red), then there should be some

[1] The Physiological Control of Judgments of Duration: Evidence for a Chemical Clock. Journal of General Psychology, 1933, 9, 267–287.

relation (however remote) between one's time sense and the gravitational constant. Indeed, Dr. W. M. Malisoff has independently, and on other grounds, strengthened the hope for such a relation in connecting the *acceleration* of chemical reaction with the gravitational field. Still further, since cosmic rays are disintegrating millions of atoms in our bodies each second, and since the resulting radiations quite possibly act on our synapses, and since cosmic "rays" may be relics of the original explosion of the universe, it may turn out that the human time sense is a function of the rate of expansion of our cosmic bubble! All this appears very complicated, but it is precisely what one would expect on an *organic* theory of nature—and, let me add, does not detract one iota from the important and interesting viewpoint which Dr. Hoagland has put forth.

Oliver L. Reiser.

Department of Philosophy,
University of Pittsburgh.

ON THE PRINCIPLE OF THE FLEXIBILITY OF SCIENTIFIC TRUTH

Sir:

Some time ago I formulated in a letter to the Physical Review[1] a new type of reasoning which is based on what I called the principle of the flexibility of scientific truth.

The Principle

For the sake of my present readers I repeat here briefly the essence of this principle. I said in my letter:

"From a deeper scrutiny of the foundations of scientific truth it follows that every scientific statement referring to observations should possess a certain minimum degree of flexibility. In other words no set of two-valued truths can be established with the expectation that this set ultimately will stand the test of experience. Formulations of scientific truth intrinsically must (should) be many-valued.

"I emphasize that the principle of flexibility of scientific truth refers to any type of symbolic statements of truth, whereby we mean only such truth as can be secured by measurements. Any other type of truth does not fall within the realm of present natural science and is therefore not subject to our discussion."

[1] F. Zwicky, Physical Review, *43*, 1031, (1933) and E. T. Bell, Physical Review, *43*, 1033, (1933).

For further explanation it must be added that the reference system of scientific truth consists of sets of *finite* numbers of experiments. An infinite number of tests I consider as non-sensical scientifically. For the non-physicist I add that scientific tests in a majority of cases may be reduced to

(a) The observation of certain coincidence conditions.

(b) The counting of numbers of appearances of certain phenomena.

The reference system of scientific truth is established by agreements, concerning operations of the type (a) and (b), between a number of persons whom we call (recognized) scientists. There are certain individuals who do not enter the above agreements. Scientists often choose to call them cranks. For cranks the reference system which underlies scientific truth is meaningless. It seems evident that the distinction between cranks and scientists is not a sharp one but admits of all gradations.

An example of a *coincidence condition* is the observation of the passage of a star through a given meridian. Or another example: the electrical resistance of a piece of wire is determined by observing the simultaneous coincidence of two pointers with marks on the scales of a volt meter and an ampere meter respectively.

An example of *counting* is the determination of the number of impulses in a Geiger counter or of the number of tracks in a Wilson cloud chamber.

Agreements between scientists regarding observations of coincidences or counts constitute the simplest of scientific facts. However, facts of this type are hardly ever mentioned in scientific publications. Scientific facts which are ordinarily presented involve a great number of additional agreements. For instance, counts are considered as of value only if the identity of the elements which are counted has been agreed upon. In turn the recognition of identities is based on the assumption of the *reproducibility* of certain phenomena. In identifying for instance elementary particles, such as electrons from cloud chamber tracks, it is *assumed* that "under the same conditions" identical particles produce identical tracks. In this connection it is of interest that recently the application of the law of identity has assumed some curious aspects. The formulation of this law by the classical systems of logic is gravely endangered through the discovery that positive and negative electrons may disappear, form hard gamma rays and subsequently divide into a score of soft light quanta.

The postulate of *reproducibility* perhaps seems to be more solidly founded. Still, a moment's reflection on certain ways of nature will

quickly shake this belief. Indeed it might be maintained that the course of events in our life and in the life of the universe in no ways encourages any belief in the absoluteness of reproducibility. If scientific thought intends to embrace the problem of evolution, reproducibility almost certainly will have to be sacrificed. In this connection it must not be forgotten that the notion of reproducibility already has undergone some important changes through the advent of quantum mechanics. This theory holds that the experimental investigation of a given phenomenon yields answers which depend on the type of experiment one performs and that the performance of identical experiments often yield answers which are only statistically reproducible.

From these few remarks it becomes apparent that a *scientific fact*, not to speak of a scientific law, is a very *complex structure of thought* and that its truth value has a meaning only with respect to a reference system of certain agreements of a body of men of science. The above considerations also indicate that there is little absolute about these agreements and that the field of eligible agreements from which a choice may be made is a wide one.

The principle of flexibility of scientific truth is based on a profound *disrespect* for the absoluteness of such truth. However, disrespect in itself is of little value. Disrespect assumes scientific value only when it becomes productive. Our judgment of its productivity depends on our definition of scientific value which may be stated as being related only to activities which advance knowledge, i.e., increase the probability of predicting correctly pointer readings, counts, etc. I therefore proposed the problem of investigating whether or not it is possible to establish a *principle of disrespect* in science which furthers our knowledge through systematic and properly selected negation. The principle of the flexibility of scientific truth is a first attempt in this direction. The considerations which lead up to this principle involve a study of the numerous possibilities regarding the choice of those agreements without which scientific thought cannot be established. As these considerations are rather philosophical in nature I did not include them in my letter to the Physical Review; neither can I go into this fundamental subject here; but I hope presently to publish a general survey of some of the major problems which concern the foundations of scientific thought. For the present the above lines may suffice to indicate that the problem of whether scientific truth is absolute or flexible is intimately related to the problem of the choice of rules which govern the scientific game.

In order to prevent misunderstanding it is of importance to remember

that the flexibility of scientific truth is not related to the fact of the limited accuracy of the experimental tests which constitute the reference system of scientific truth. Absolute scientific truth is unattainable regardless of the agreed upon margins of accuracy of observations, as long as these margins are finite and definite.

Some critics at this moment probably will stumble into the pitfall of pointing out that I myself make absolute statements in maintaining the principle of the flexibility of scientific truth. Such critics I wish to remind that this principle is scientific in nature only insofar as the predictions derived from it constitute scientific statements which can be tested by a finite number of experiments. The negation of the principle however is not scientific in nature inasmuch as the verification of this negation would require an infinite number of experiments. Through this double aspect the principle safeguards itself from being annihilated through its own tools after the fashion of Epimenides the Cretan. Any criticism which negates the principle as a whole must be metaphysical in nature.

Purpose of the Principle and Examples

Metaphysical thought, admittedly or not, has always played a great rôle in bringing about the fundamental advances of science. A beautiful example of this contention is the belief, recently expressed by A. Einstein,[2] that "in Nature there is actualized the ideal of mathematical simplicity." Although this belief admits of no deduction and although it has never been formulated rigorously, it has been instrumental in scientific creations of the past and probably will stimulate discoveries in the future.

The postulate of the flexibility of scientific truth may be regarded as another suggestion of how to track down the obscure doings which produce intuitive discovery. This postulate is a first systematical attempt to deliver philosophical thought from its age old tendency to indulge in hair splitting discussions of self created difficulties and to turn at least a part of philosophical thought into a scientifically fruitful procedure.

Philosophers early recognized that everything is subject to change. Nevertheless the metaphysical desire to save the absoluteness of *identities* made itself manifest down through the ages. New and everchanging attempts were made to interpret all changes as the varying aspects of essentially *absolute elements* which for ever preserve their identities.

[2] A. Einstein, Philosophy of Science, Vol. 1, No. 2 (1934).

Democritus created the absolute and unchangeable atoms. One recognizes a parallel to this attempt to save the absoluteness of identities in Einstein's desire to save the absoluteness of simplicity.[3] At the end of the eighteenth century Democritus' dream took on concrete form. But the dream of absoluteness of identities was short lived. It was soon recognized that atoms change, appearing some time as neutral particles and some time as ions. The belief in absoluteness persisted and endowed the nuclei of atoms and the electrons with invariability. The nuclei in turn soon proved disappointing. Natural radioactivity and artificial disintegration demonstrated the variability of nuclei. And alas, recently one of the last mainstays of absoluteness fell with the probable creation from gamma rays and annihilation into gamma rays of positive and negative electrons. All statements about the identities of electrons have herewith been reduced to *statistical* laws in keeping with the predictions and requirements of the principle of flexibility of scientific truth.

Is there any absoluteness left? Yes, we are still in doubt about the identities of the proton and the neutron. Also, annihilation involves so far only pairs of positive and negative electrons, which means that the total charge of a closed system is invariable and degrees of freedom of matter can be changed in pairs only. The principle of the flexibility of scientific truth predicts that these remains of absoluteness of identities will not survive for very long. We must expect that individual electrons and individual charges will prove subject to annihilation. In which direction we have to search for such effects I have already sketched in my letter to the Physical Review.

From the above example we see that the negation of absoluteness is always accompanied by the discovery of new phenomena. Negation, if properly handled may scientifically be highly constructive. In mathematics negation has long since been systematized into constructive principles. In physics this task is still before us.

[3] Perhaps one might argue that the beliefs in simplicity and absoluteness are related to a fundamental desire of certain men for beauty. This desire has not always found its realization in the ordinary ways of life, and man by wishful thinking endows at least science with simplicity and beauty. Although I take the stand that orthodox and absolute formulations of truth are ultimately inadequate for a comprehension of all being (German "sein"), I am of course fully aware of the fact that the belief in absoluteness was and still will be extremely fruitful and perhaps necessary in man's approach toward a comprehension of all being.

Criticism

Recently Mr. H. Margenau has attacked the validity of the principle of the flexibility of scientific truth in these columns.[4] As the considerations which led me to this principle are still unpublished, Mr. Margenau's criticism perhaps is somewhat premature. He bases his criticism on the following claims.

(1) That I am trying to derive my principle from some system of many-valued logic.

(2) That there are any number of propositions which are certainly true or false.

(3) That the specific applications which I have derived from the said principle refer to difficulties which can be resolved otherwise.

Contention (1) is incorrect. In establishing the scientific game it is necessary to agree which rules of thinking shall be adopted. Logic itself is subject to this choice. I therefore have never even suggested that the principle of the flexibility of scientific truth can be derived from any system of many-valued logic. In particular I have left no doubt in my letter to the Physical Review that rules of logic such as those referring to identity, negation, excluded middle etc. admit of many fundamentally different formulations. The so-called laws of logic are therefore themselves subject to the flexibility of scientific truth and not vice versa.

Contention (2) can perhaps best be answered by the challenge that Mr. Margenau actually produce a proposition which scientifically is certainly true or false. I propose to discuss in the light of the principle of the flexibility of scientific truth any such proposition which Mr. Margenau chooses to advance.

Contention (3) is hardly subject to any discussion as it is up to future experimentation to decide whether or not the numerous predictions which can be derived from the principle of flexibility of scientific truth will be realized.

F. Zwicky.

Norman Bridge Laboratory of Physics,
California Institute of Technology,
Pasadena, California.

[4] H. Margenau, Philosophy of Science, Vol. 1, No. 1 (1934).

MEANING, ASSERTION AND PROPOSAL

Sir,

I am glad that the questions of Dr. Dewey afford me the opportunity of giving more detailed explanation of two points in my paper "*On the Character of Philosophic Problems.*"

1. I have to confess that what I said about *the formal and the material* (inhaltlich) *modes of speech* could not give a clear idea of the matter. The exact definition of the two modes could be given neither in that paper, nor here, because concepts which are too technical are necessary. The exact definition is given in my book "Logische Syntax der Sprache" (Wien 1934). But I will try to explain the difference a little more explicitly.

I distinguish three sorts of sentences:

(a) *Real object-sentences.* [They deal not only seemingly but actually with extra-lingual objects.] Example: "The rose is red."

(b) *Pseudo-object-sentences* or sentences of the *material mode of speech.* [They deal seemingly with extra-lingual objects, e.g. the rose, but actually with the lingual signs of those objects, e.g. with the word 'rose.'] Example: "The rose is a thing."

(c) *Syntactical sentences* or sentences of the *formal mode of speech.* [They deal with lingual expressions.] Example: "The word 'rose' is a thing-sign."

The explanations in square brackets are somewhat inexact. A little more exactly formulated the definition is as follows: a sentence ascribing a certain quality $\mathfrak{Q}_1$ to an object belongs to the *material mode* of speech, if there is another, namely a syntactical quality $\mathfrak{Q}_2$, which is parallel to $\mathfrak{Q}_1$. By a *syntactical quality* $\mathfrak{Q}_2$ being parallel to $\mathfrak{Q}_1$, is meant that when and only when an object possesses the quality $\mathfrak{Q}_1$, a sign of this object possesses the quality $\mathfrak{Q}_2$. In the examples given above "being a thing-sign" is a parallel syntactical quality to the quality of "being a thing," because when and only when something is a thing, its sign is a thing-sign. Therefore the sentence "the rose is a thing" belongs to the material mode of speech. This sentence can be translated into the parallel sentence " 'rose' is a thing-sign," belonging to the *formal mode* of speech. On the other hand there is no syntactical quality which is parallel to the quality of "being red," i.e. no syntactical quality possessed by the signs of all things that are red and only those; because we cannot decide the question whether a certain thing is red or not by observing only the sign of that thing. Therefore the sentence "the rose is red" does not belong to the material mode of speech, but is a real object-sentence.

Thus the distinguishing of the formal and the material modes of speech concerns only those sentences which deal with language, whether this fact is manifest (in the formal mode) or veiled (in the material mode).

As to the question raised, the relation between empirical matter and the material mode of speech is the following:—empirical matter is dealt with in real object-sentences while sentences of the so-called material mode of speech do not concern empirical matter, they only seem to do so; they could therefore be called more clearly pseudo-material.

2. *Difference between assertion and proposal.* Example of a syntactical *proposal:* "'$p \supset (p \vee q)$' is to be an axiom of the language *L*." This proposal is a part of the definition of '*L*' (the name of the proposed language). Example of a syntactical *assertion:* "In the language *L* the sentence '$p \equiv (p \vee q)$' is demonstrable." This assertion only has sense if the name '*L*' has been previously defined (by introducing the axioms and rules of inference). The answer to the question whether a given syntactical sentence S_1 is a proposal or an assertion depends upon whether the language-name which occurs in S_1 has been previously defined or not. (This latter remark is valid only for sentences of such simple form as the examples mentioned here.) Consequently I should think that a sentence S_2 which says that a given syntactical sentence S_1 is a proposal (or that it is an assertion) can be determined objectively either to be true or to be false; provided of course that the sentence S_1 as well as the whole connection to which it belongs is given in a perfectly correct and complete form. It seems to me therefore that the sentence S_2 is not itself a proposal but an assertion.

R. Carnap.

Prague, XVII,
N. Motol, Pod Homolkou.

REVIEWS AND NOTES

THE FEELING OF FEELING

The Philosophy and Psychology of Sensation, by Charles Hartshorne. University of Chicago Press, pp. xiv + 288, 1934.

The progress of science has never been smooth and easy, over a path that is straight and free from obstacles. History shows that there are times when its progress stops, when it seems to go back to look over the ground and find a new way to get around what is proving a difficult stretch. One of these ways it has tried time and again, with a degree of success it is hard to evaluate. What seems to happen is this, that science goes ahead, and along the path on which it was started. Its little detour seems to help, in some way or other, perhaps by providing a breathing-spell, but never sends it off in a new direction.

The normal course of science, at least so history suggests, is that marked out by concepts susceptible of measurement. It deals with quantities and the ways in which they vary. Its aberrations occur when this normal course seems blocked, and those wills-of-the-wisp, the qualities, appear to show a new way. Though it has never proved a good way, we are tempted to follow it still, and do, until a clearer light appears to show us how to deal with our subject in the only way we really can.

The work now before us represents a yielding to temptation. It looks at current theories of sensation and sees them standing still, the forward path apparently closed, attempted alternatives leading nowhere. And it suggests a face-about, and a tentative forage in an entirely new direction. It proposes to abandon the quantitative treatment of the data of sensation, and find some way to analyze those qualities in which it presents itself.

This is no easy way, to be condemned as such. In fact, always in the past it has proved too hard, if not inherently unfruitful. Nor shall we condemn it beforehand for that lack of success which history teaches we can expect of it. For it may be inevitable, this periodic giving up and

starting out and coming back again. Perhaps it is the only way to get ahead. And science has progressed, according to the standards which it has set itself.

A departure from the familiar and accepted must excuse itself. This work, then, must show that past and current efforts toward a science of sensation have reached an *impasse*, that the difficulties they face are such as to justify the proposed turning aside. These difficulties are, in the main, due to one thing, the failure to apply the "currently accepted principles of scientific research, as well as the most characteristic ideas of contemporary philosophy." And the difficulties are found to be of three sorts. First, current theory, which is condemned as materialistic, violates in some twelve ways the scientific ideal of theoretical simplicity. It is filled with complexities, the first and most fundamental of which results from its acceptance of the Helmholtzian dictum that "intelligible comparison of qualities is to be confined to qualities given by the same class of organs." This is taken to be opposed to the principle of mathematical continuity which must characterize sound science. Other unnecessary complexities arise as violations of this and four other principles which are laid down when five fundamental conceptions, which are to characterize the new theory, are treated as requisite to a scientific treatment of the subject. These conceptions, to be considered in connection with what is supposed to be constructive in the work, will be dealt with later. They are as follows: (1) mathematical *continuity*; (2) aesthetic meaning or *affective tone*; (3) the fundamentally *social* character of experience; (4) biological *adaptiveness*; (5) *evolution* from a common origin. Unity is to be enjoyed by adhering to them, complexity must result from ignoring them. And there is no need to accept as an axiom, as current theory has, the proposition that 'sensory qualities are inexplicable' because ineffable and ultimate.

The second sort of difficulty with current theory is this, that it is frequently in conflict with observable facts. The facts cited are of two sorts. Some are related to the affective or emotional aspect of sensation, that is to the concept most fundamental to this new thought, the *feeling of feeling*. These facts are of course of the immediate sort, of which it is asked, 'Who does not know that this is the case?'. A quotation will serve to point to an instance of a less popular sort than most. "It has been found, moreover, that the most accurate matchers of colors are aware that they achieve this success by referring in their judgment to the emotional tone of the colors. This is scientifically respectable evidence for two things: . . ." (p. 121) (A footnote points out that the

information is derived from a work whose author does not give the source of his statement.) Facts of the other sort are concerned with the interrelations of the sense qualities associated with various organs. The particular instance which is most convincing is that of brightness and pitch. " . . . the perception of the brightness of tones, if it can be taken at its face value, is simply the demonstration that so far at least as this dimension is concerned color and sound are qualitatively alike." (p. 59)

The facts that John Burroughs calls the song of the wood-thrush "golden" and others agree that the hermit-thrush's song is "silver" are taken to be relevant. "We have to face, after a century of evasion, the general question: Are the distinctions of kind recognized between different phases of experience, such as vision and hearing, such as sensation and affection—are these distinctions the final word, or does the plain man, any man not psychologizing, betray a deeper insight when by employing the "metaphors" of speech, he implies an underlying homogeneity of experience such that sound can really be "sweet" or that sounds, colors, feelings, and temperature sensations can all be "warm" or "bright"? Science rests upon the discovery of unity; it is therefore a paradox that the speech of untrained men should postulate far more radical unities of the mental life than scientific psychology as yet recognizes. Wherever such common-sense comparisons exceed in this fashion the unity now conceded by science, I predict that psychological, based on physiological, homogeneities will sooner or later be revealed experimentally." (p. 73)

A third reason why current theory is in a state which warrants its abandonment is its failure to produce agreement among those who propound it. Two chapters demonstrate the extent of this disagreement. Philosophers disagree because they fail to take proper account of the data. They fail for two reasons: because they are incapable of separating the data from knowledge about the data, and because they are ignorant of the results of introspective psychology. Psychologists disagree because of their materialistic tendencies. Introspectionism and behaviorism are opposed. "Either introspectionism must be given up, or sensory materialism must be discredited. . . . The future of introspectionism is bound up, it begins to appear, with the future of the affective continuum." (p. 108)

This mention of the 'affective continuum' brings us back to the constructive part of the work, since the chapter on "Dualism in Aesthetics" is one with which the reviewer is incapable of dealing. This constructive part is to be regarded as highly tentative, the author says in his preface. This is made even more evident in the concluding chapter,

where it is predicted that most of the points which are argued in the text will be found to be so.

The 'affective continuum' is the backbone of the proposed theory. The principle of continuity requires that experience be essentially social. And social feeling, or sensation, since that is the topic with which the book is dealing, is nothing but 'feeling of feeling.' It is the 'affective tone' of sensation with which science should deal. There are four ways in which this may vary, four dimensions, that is, which define the affective continuum. They are these: self-other, active-passive, positive-negative, faint-intense. And this affective continuum may be taken to be all-containing. "The world may be conceived as the increasing specification of the theme 'feeling of feeling'." (p. 208) Success in dealing with its dimensions will relieve philosophy and psychology of some of their most persistent bug-bears. "If such things as sense qualities can be described in terms of them, then it is not true that the redness of one person's experience might be different from that of another's without any possibility of verifying the fact." (p. 196)

Of these dimensions, it is the last which is most readily dealt with. Brightness or intensity has a quantitative aspect which removes some of the mysteriousness that has always belonged to qualities. The difficulties which the others encounter might be avoided, it is suggested, by embracing a panpsychic doctrine—taking "seriously the view that a cell is not only a living but a sentient organism." (p. 244) This sort of doctrine is taken to provide an answer to the question "If a sensation is a mode of immediate intuition, of what is it immediately aware?" The answer is this, "If anything is directly known to be qualified by redness, it is one's own body when seeing red." "... what we intuit as red is the state of our own nerves together with the state of the environment ..." (p. 246).

A great deal is made of the possibility of a relationship between sense qualities and the adaptation of the individual to his environment. The following passage is typical: "In short, the green vegetable covering of the earth is just the normal fact of existence. Psychologically, then, we should expect green to appear as a feeling of quiet cheerfulness. Who does not feel this to be in fact the character of green?" (p. 254)

In the preface it is stated that in the text "The problem of the nature of mind is reduced to a conception of variables of 'feeling of feeling,' social feeling as such, and their possible values." On p. 194, after a discussion of the social continuum, there follows this: "The point is that there are two possible views: either mind or awareness as such is logically dependent

upon other mind or awareness, or it is not. The latter view has been tried. It has led to all the paradoxes of epistemology, and to a disastrous attempt to found duty upon self-interest. The other view has scarcely been tried at all." One would like to remind the author that for years Professor Singer has insisted that the existence of a mind depends upon the existence of a mind to be aware of it, that he has been able to deal with "mind as behavior" without recourse to so-called materialistic doctrine, and that he has been able to defend his thesis systematically.

The author concludes with still further prediction. There are three possibilities in the future of science: (1) that the various natural sciences will remain distinct, (2) that physics will absorb both biology and psychology, and (3) that "psychology, suitably generalized to include all organisms—electrons and all known individuals being so classified—may absorb biology and physics." (p. 268) The last is most probable.

But a more fitting conclusion to this review might be found in these passages: "There seems but one justifiable verdict—the question calls for further study, particularly, perhaps, experimental study. It may be also that we need to form clearer conceptions than anyone now has concerning the logical aspects of qualitative continuity and discontinuity, concerning the nature of qualitative comparison." (p. 242) "Perhaps the problem of immediacy is too delicate for experimental investigation? But who shall say how far our methods might be refined?" (p. 154)

Chester Townsend Ruddick.

University of Pennsylvania.

Note: Lest it appear from this review that the author of the work bases his argument entirely upon popular experience and dialectic, it should be remarked that there are copious citations of experimental and other studies by numerous men. Those to whom particular indebtedness is acknowledged in the preface are the late L. T. Troland, Professor E. G. Boring, and Julius Pikler.

A NEW BUDGET OF PARADOXES

(3) CLASSIFICATION OF PARADOXERS (*Continued*)

Class II. Phantom-Criers
A. Creators of Straw Men
B. Alarmists

Who are the phantom-criers? Their origin is ancient and dishonorable. The original phantom-crier was the one who first made someone else see a ghost. In this connection pre-Adamite possibilities should be considered. We, however, are interested in the modern descendants, who have subsisted on highly civilized fears.

The group, creators of straw men, have as their objective, of course, the knocking down of these straw men, presumably thereby earning the applause and the gratitude of the frightened spectators. The group, alarmists, on the contrary, are not interested in permanently allaying a fear of their making, for that would hurt the sale of their sedatives.

Straw men are easily created in the heat of argument. Exaggerate the assertions of an opponent—so much straw. Piece it all together—a straw man! Then set a match to it in irony. Certain types of self-appointed defenders of culture are adept at constructing straw-scientists, who are automatons far below the level of robots, and whose downfall all friends of humanity must desire. There's only one pesky difficulty,—they don't exist. Even Tolstoy indulged himself in such a performance when he declared all too seriously that the scientist's mission is to count the number of lady-bugs on this planet. There are battalions of such straw philosophers, kinsmen of the "absent-minded professor." There's a whole regiment of straw-Einsteins. Yet, in fairness be it said, lesser men are involved in phantom battles too. The "average man" is perhaps the most patient of all straw men.

Alarms are created with some difficulty. One must get mass action,—it is necessary to have at least a "movement," a nucleus which spins and centrifugally sends streamers of alarm through the sluggish gel of the body politic, till in due time it too rotates and churns. Everyone

repeats the formula, the formula of the alarm. Thus, it is a mighty chorus that now chants the humanistic alarm over the Machine Age. Naturally strange cures are seized upon in the goose-stepping frenzy. The Decline of the West is to be stemmed by the triumph of Hitler-Aryans, machines are to be consigned to the devil of rust, and the greedy fields of science are to be left unplowed for a decade or more. Fortunately, however, most alarmists are no more harmful than the German divine of a quarter of a century ago, who, believing the earth to be flat, petitioned the Kaiser steadily to make it a capital crime to be a scientist.

W. M. M.